THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXII. — *On the New Action of Magnetism on a Permanent Electric Current;** by E. H. HALL, Assistant in Physics at the Johns Hopkins University.

IN the early part of last winter there was published in the American Journal of Mathematics† an account of some experiments which prove that an electric current, as distinguished from the conductor bearing the current, is acted upon by magnetic force in a manner altogether different from that in which ordinary induction is known to take place. The new phenomenon was in short the action of a permanent magnetic force on a permanent electric current. Up to the time when the above-mentioned article was written this new action had been observed in only one conducting material, gold. In the present article will be given the results of observations with several other conductors; but first it seems worth while to give some account of various closely related experiments which, though resulting negatively, are not entirely devoid of interest.

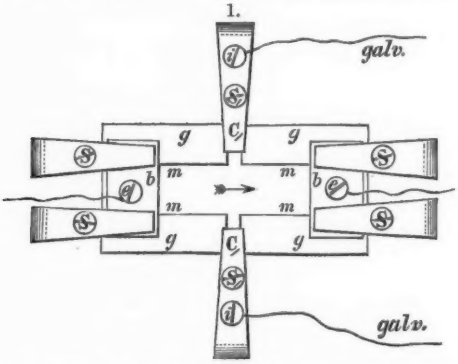
In the previous article the fact was mentioned that a form of apparatus had been devised which, it was thought, might reveal the new action in the shape of an increase of resistance in the conductor. The plan, as modified in accordance with a suggestion of Professor Rowland, was to employ as the conductor to be experimented upon a circular disk of gold leaf, in which the current entering at the center would radiate to a thick ring at the edge and so pass off by a wire attached to the ring. In such an apparatus under ordinary conditions the electromotive force, and so the flow of electricity, would be

* In its original form this article was a thesis for the degree of Doctor of Philosophy. Some alterations have been made in preparing it for publication.

† Vol. II, page 287, 1879; republished in this Journal, March, 1880.

along the radii of the disk, but if a strong magnetic force were made to act perpendicularly to the face of the disk a new electromotive force would be set up, which would be always perpendicular to the direction of the magnetic force and to the actual direction of flow of the electricity at any instant in every part of the disk. The actual electromotive force therefore, under which the electricity would flow, would be compounded of two, one of which would in general have the direction of the radii of the disk, while the other would be nearly at right angles to this, though changing its direction constantly as the flow of electricity continually veered from its normal course under the resultant action of the two electromotive forces. The resulting path of the electricity from the center to the circumference of the disk would be, not a straight line as under normal conditions, but a spiral. This path being longer than the straight line, we should expect an increase of electrical resistance in the disk of gold leaf. Before any very extended experiments had been made with this apparatus, however, it was pointed out by Professor Rowland that the increase of resistance which might be looked for in this case would be exceedingly small, too small probably to be detected. This experiment was therefore abandoned for the time at least.

The next experiment to be described was a very simple variation upon the main one, and before going farther it may be well to give a drawing of such a plate as has been used in making most of the observations to be hereafter recorded.

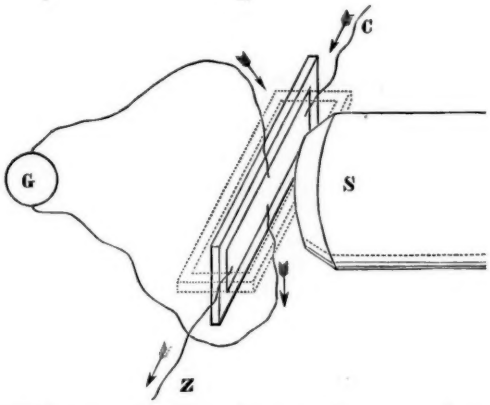


In fig. 1, which is about one-half the actual size of an ordinary plate, *g g g g* represents the plate of glass upon which the metal strip *m m m m* is mounted. Contact with this strip is made at the ends by the two thick blocks of brass *b, b*, which are held firmly in place by the four brass clamps worked by means of the screws *S, S, S, S*. The main current of electricity

enters and leaves the metal strip by means of the binding screws *e*, *e*. Running out from the middle of this strip are two projections which make contact with the clamps $C_{,}$, $C_{,}$, worked by the screws $S_{,}$, $S_{,}$. From the screws *i*, *i*, wires lead to the Thomson galvanometer. The projections from the metal strip just alluded to make the apparatus very easy to adjust, for by scraping off little particles from the proper part of the projections, while the current is allowed to run through the metal strip, the current through the Thomson galvanometer may be reduced to the extent desired.

In ordinary experiments such a plate as that just described is placed between the poles of the magnet in such a position that the direction of magnetic force would be represented by a perpendicular to the plane of the paper in the above drawing.

2.



In the variation upon the main experiment a plate was employed similar to the above, but narrower and with very short side clamps. This plate was first placed between the poles of the magnet in the usual position as shown by the heavy lines in fig. 2.

With this arrangement a permanent deflection of about 30 cm. on the scale before the Thomson galvanometer could be obtained by reversal of the magnet current. Leaving now the distance between the poles very nearly the same as before and using, both in the magnet and the gold strip, as nearly as possible the same strength of current which had just been employed in the previous trial, the plate was turned into the position indicated by the dotted lines in fig. 2. With this second arrangement no action of the kind previously seen was detected, or at least none that could with certainty be distinguished from the direct action of the magnet on the Thomson

galvanometer. This latter effect produced a deflection of only a few mm. and could not have masked any considerable action of the kind looked for.

The first part of this experiment then shows our main fact, viz: that in a conductor subjected to the given conditions a permanent electromotive force is at once established which has a direction perpendicular to the direction of magnetic force and perpendicular to the direction of the primary current in the conductor. The second part of the experiment shows that under similar conditions no electromotive force is set up in the direction of the magnetic force, or at least none of the same order of magnitude as that described above.

The third experiment to be described was made at the suggestion and desire of Professor Rowland. It was to test for an action of the magnet on the lines of static induction in glass. A thick piece of plate glass about four cm. square was taken and a hole about four mm. in diameter was drilled through each of the four lateral faces. These four holes were all directed toward the center of the glass and each extended to within about seven mm. of this point. If the holes had met they would have formed two cylindrical channels at right angles to each other and extending straight through the glass from lateral face to lateral face. In each hole a loosely fitting plug of brass several mm. long was placed and securely fastened with a cement of insulating material. Leading out from each plug was a wire which was insulated for some cm. by being surrounded with a glass tube. The piece of plate glass thus prepared was placed between the poles of the magnet, precisely as a plate bearing a strip of gold would be. One of the brass plugs was placed in connection with the inner coating of a battery of Leyden jars charged by means of a Holtz machine, the opposite plug being in connection with the outer coating of the jars and with the earth. The other two plugs were placed in connection with separate quadrants of a Thomson electrometer. The quadrants were both insulated from the earth. The electrometer was sufficiently sensitive to deflect the spot of light about 170 mm. for the electromotive force of a Bunsen cell, or 340 mm. on reversing the connections with such a cell. The battery of Leyden jars was charged to a potential sufficient to give a spark of two or three mm. The connections being thus made, the position of the spot of light was observed and the magnet then operated with the purpose of discovering, if possible, any consequent change of position of the spot of light which would indicate an action of the magnet on the lines of static induction in the glass. The observation failed to establish the existence of any such action. The electrometer being in a very sensitive condition the spot of light was rather unsteady, so that any

very slight effect of the kind looked for would not have been detected, though it is probable that if a reversal of the magnet had caused a change of four mm. in the position of the spot of light, this effect would have been apparent.

We may therefore conclude that any change of relative potential on the quadrants of the electrometer caused by reversal of the magnet was probably less than $\frac{1}{80}$ of that caused by reversing the connections of the electrometer with a Bunsen cell, as mentioned above. If now we estimate the difference of potential between the plugs A and B, connected with the Leyden jars, to have been, as indicated by the length of the spark, equal to that which would be produced by 10,000 Bunsen cells in series, we may conclude that any difference of potential between the other plugs C and D which was caused by the action of the magnet, must have been less than $\frac{1}{800000}$ of the difference of potential between A and B. We must remember, however, that any change of potential on C and D had to be extended as well over the comparatively large area of the electrometer quadrants. Professor Rowland has roughly estimated the capacity of the quadrants as twenty times that of the plugs C and D. If, therefore, these plugs had not been attached to the electrometer, any difference of potential between them due to the action of the magnet would have been twenty times as great as in the actual case, so that instead of $\frac{1}{800000}$ we have $\frac{1}{40000}$ of the difference of potential of A and B as the superior limit of the difference of potential of C and D which the magnet might possibly have produced, if C and D had not been connected with the electrometer. Representing the former difference of potential by E, the latter by E′, and the strength of the magnetic field, about 4000 (cm.-grm.-sec.), by M, we have for this case of static induction in glass $\frac{E'}{E \times M}$, if not zero, is less than $\frac{1}{1800000000}$.

Turning to the analogous case of current electricity in the various metals and representing now by E the difference of potential of two points a centimeter apart in the direction of the current, and by E′ the difference of potential of two points a centimeter apart in a direction at right angles to that of the current, while M has the same signification as before, we may write, as a very rough estimate for the case of iron, $\frac{E'}{E \times M} = \frac{1}{1000000}$, while for tin the value of this ratio may be as small as $\frac{1}{600000000}$. We may therefore conclude that the equipotential lines in the case of static induction in glass, if affected at all by the magnet, are affected much less than the equipotential lines in the case of a current in iron, but we can not say that any such possible action in glass has been shown

to be smaller than the analogous action in the case of a current in tin.

I now go on with an account of further investigation of the phenomenon actually discovered and already in some measure described in my previous article. When writing that article it seemed to me instructive to deduce the ratio, $\frac{E}{E'}$, of the difference of potential per cm. on the longitudinal axis of the gold leaf strip to that per cm. on the transverse axis. There were thus obtained for the experiments made values of $\frac{E}{E'}$, ranging from 3,000 to 6,500 according to the strength of the magnetic field.*

At that time I supposed that the ratio $\frac{E \times M}{E'}$ would prove to be a constant, not only for different strips of one metal, but for all conductors. Subsequent experiments showed that this was not the case, and in this article the results obtained will be expressed by the ratio $\frac{M \times V}{E'}$, where E′ has the same signification as before, while M now expresses the strength of the magnetic field in cm.-grm.-sec. units and $V = \frac{C}{S}$ †, the strength of the primary current divided by the area of section of the conductor. This ratio does not prove to be the same constant for different metals but for any particular metal it seems much more nearly a constant than the ratio $\frac{E \times M}{E'}$ given above would be.

It may seem to those who read the following pages that an unnecessary amount of study has been devoted to gold. It must be remembered, however, that many readers of my previous article were not fully convinced by the evidence there adduced that any really new principle had been discovered, thinking that the explanation of the phenomenon described was possibly to be found in some such fact as the state of mechanical strain, into which the strip of gold leaf would be thrown in its endeavor to move across the lines of magnetic force in obedience to the perfectly well known laws of the

* In obtaining this latter quantity, which was called M, a serious error was made and the value given was probably not much more than half what it should have been. This fact was mentioned in a note when the article in question was republished in this Journal, Mar., 1880, p. 200, and p. 235.

† This quantity V may be said to bear an intimate relation to the absolute velocity of the electricity, for if we were to take as the unit velocity of electricity that of a unit current flowing through a conductor of unit cross-section, the velocity in any particular case would be a quantity $\frac{C}{S}$.

action of magnets on conductors bearing currents. This being the case, it seemed desirable to make experiments with several strips of the same metal and determine whether the ratio $\frac{M \times V}{E'}$ would prove to be a constant for all. The dimensions of many of the strips used, of whatever metal, are given below, and in order that the conditions to which they were variously subjected may be more fully understood, there will be given in many cases the strength of the magnetic field in absolute units and the strength of the primary current through the strip, the latter being expressed in terms of the constant, k, of the tangent galvanometer used to measure it. This constant there has been no occasion to determine exactly, but it is about ·07.

It will probably be readily admitted that the results obtained cannot be accounted for without admitting substantially all that was really claimed in the previous article. Even if no such quantitative investigation had been made, however, there would still be one fact inexplicable on the theory of an accidental cause for the phenomenon under consideration. The arrows in fig. 2 show the direction of the transverse current relatively to the direct current in gold, the magnet pole, S, being a south pole, i. e., the pole attracting the north pointing end of a needle. This relation between the directions of the two currents and the magnetic force is the same in all of the four gold plates which have been examined in this particular. The same uniformity is observed in the four silver plates, and the three iron plates, which have been tested in the same way. With the two plates of tin which have been examined there has been a trifle of uncertainty upon this point, as the effect in this metal is at best very small, but this uncertainty is hardly sufficient to cast doubt upon the correctness of the rule that, so far as observation has gone, the relative direction of the transverse current is always the same for any particular metal. This uniformity in so many cases could hardly be accidental.

This matter of direction is evidently one of fundamental importance. The direction was found to be the same for silver as for gold, these being the two metals first examined. Professor Rowland, however, predicted that the direction would be reversed in iron and experiment verified the prediction. Prof. Rowland's comments upon the significance of this discovery are already before the public*. It is a seemingly awkward fact that in nickel, next to iron and cobalt the most strongly magnetic substance, the direction of the transverse current is the same as in gold. This fact will be discussed further on. The conductors which have, up to this date, been subjected to ex-

* Am. Journ. Math., vol. ii, p. 355.

periment are gold, silver, iron, tin, nickel and platinum. The direction is the same in all except iron.

The extreme irregularity in the results obtained in the early part of this course of experiments was due to various causes, only one of which is worth mentioning here. This source of error was the shape of the magnet poles, which, being intended for the study of the magnetic rotation of polarized light, were perforated axially by a hole several mm. in diameter. With these poles the magnetic force was found to vary many per cent in different parts of the field. These poles were subsequently replaced by solid ones, and a sufficiently uniform field was thus secured. It will, however, be noticed that even after this change the results obtained on the same day and with the same plate often vary by several per cent. Probably quite a part of this irregularity was due to the faulty manner in which the tangent galvanometer, which measured the strength of the primary current through the strip, was introduced. This source of error can probably be avoided in future measurements. Again it is to be remembered that the strength of the transverse current was determined by a delicate Thomson galvanometer, an instrument far more sensitive than accurate. In using comparatively thick strips of metal there is especial liability to error from this source, as a low resistance galvanometer must then be employed, which may easily change in sensitiveness several per cent within an hour.

Much of the disagreement to be observed in the results obtained with different plates of the same metal, is no doubt to be explained by the difficulty of determining with anything like accuracy the thickness of the various strips employed. I have tried to determine approximately the thickness of the thinnest films used by measuring the electrical resistance, but this method, as will be seen, is exceedingly faulty. The thicker strips have been weighed before being placed on the glass, but even this method fails to determine the effective thickness accurately. Even if the specific gravity were the same for all the strips, and it probably is not, the value thus obtained for the thickness would give only the average thickness, and this is by no means the effective thickness. It will be remembered that the connections leading to the Thomson galvanometer are placed opposite to each other with the width of the metal strip between them. The effective thickness is the average thickness along the line joining these two side connections. Gold foil is obtained in sheets ten or twelve cm. square. It will be seen further on, that in one case two strips cut from similar positions in the same sheet differed in average thickness about seven per cent. This being the case it seemed quite possible that the effective thickness of any strip, as defined above, may

differ many per cent from the mean thickness indicated by the weight.

All these sources of error being considered, the discrepancies which will be observed in the results to be given, will not be surprising.

A single complete series of observations consisted of the following parts:

1st. A determination of the extent to which the indicator of the Thomson galvanometer was affected by the direct influence of the magnet and the magnetizing current.—All that it was necessary to ascertain in this case was the change in position of the galvanometer indicator caused by reversing the current through the magnet. This usually amounted to one or two mm. and subsequent readings of the Thomson galvanometer were, when it was necessary, corrected accordingly.

2nd. A determination of the strength of the magnetic field.—This was done by withdrawing suddenly from the field a small coil consisting of a few turns of wire and observing the effect of this action on a delicate galvanometer placed in circuit with the coil.* The galvanometer was used with a mirror and scale and the readings actually obtained were reduced by the formula

$$\sin\frac{\Phi}{2}=\frac{n}{4r}\left(1-\frac{11}{2}\left(\frac{n}{4r}\right)^2\right)$$

where n is the actual reading and r the distance from the mirror to the scale. The constant of the galvanometer not being known, its sensitiveness, that is the significance of its readings in absolute measure, was determined whenever the strength of the magnetic field was to be found. This was effected by means of an earth inductor placed in circuit with the galvanometer and the test coil used with the magnet. The determination of the strength of the magnetic field therefore involves two series of observations, one with the earth inductor and one with the test coil.

3d. A determination of the sensitiveness of the Thomson galvanometer.—This was done by sending through it a current of known strength obtained by shunting the current from a Bunsen cell, the main current being measured with a tangent galvanometer.

4th. The main experiment.—The primary current through the metal strip measured with the tangent galvanometer just spoken of, and the effect of reversing the magnet observed on the scale of the Thomson galvanometer.

5th. Another determination of the sensitiveness of the Thomson galvanometer.—Method as described above.

*Rowland, "On a Magnetic Proof Plane," this Journal, vol. x, p. 14, 1875.

6th. Another series of observations with the test coil.

7th. Another series of observations with the earth inductor.

8th. Another determination of the direct action of the magnet on the Thomson galvanometer.

If, as was usually the case, several series were to be made with the same plate in one day, for the purpose of using primary currents of various strengths, the sensitiveness of the Thomson galvanometer was tested before each main series of observations and after the last.

The mean of two values found for the sensitiveness of the Thomson galvanometer was of course taken to be the sensitiveness during the series of observations intervening. It was not found necessary to determine the strength of the magnetic field more than twice during a half day's observations.

In working up these observations the following formula applies:

$$\frac{M \times V}{E'} = \frac{7460\, H \dfrac{\sin \dfrac{\Phi}{2}}{\sin \dfrac{\Phi'}{2}} \dfrac{k \tan \alpha}{wt}}{\dfrac{dk \tan \Theta\, pr}{d'w}}$$

M, V, and E′ have been already defined.

7460 = twice the integral area of the earth inductor divided by the integral area of the test coil. Twice the simple ratio of these two areas is taken, for the reason that the earth inductor coils are turned through 180° when used.

H = horizontal intensity of earth's magnetism at position of earth inductor.

$\sin \frac{\Phi}{2}$ = a quantity relating to effect on the galvanometer used with test coil, produced by withdrawing the latter from the magnetic field.

$\sin \frac{\Phi'}{2}$ = a similar quantity relating to the galvanometer and the earth inductor.

k = constant of tangent galvanometer.

α = reading of tangent galvanometer when measuring primary current through the metal strip.

w = effective width of metal strip.

t = effective thickness of metal strip.

d = difference in readings on the Thomson galvanometer scale caused by reversing magnet in the main experiment.

d' = difference in readings on same scale caused by reversing current in determining sensitiveness of the Thomson galvanometer.

Θ = reading of tangent galvanometer when measuring current used to determine sensitiveness of Thomson galvanometer.

p = proportion of above current which passes through the Thomson galvanometer.

r = total resistance of circuit containing Thomson galvanometer during main experiment.

The above formula reduces to the form

$$\frac{M \times V}{E'} = \frac{7460 \sin \frac{\Phi}{2} \tan \alpha \, d' \, H}{t \, d \, p \, r \tan \Theta \sin \frac{\Phi'}{2}}$$

It will be seen that k and w have disappeared. The elimination of w is a very important fact as this would be an exceedingly difficult quantity to determine with accuracy. As the case stands, it is not at all important to preserve the form of the metal strip after its thickness has been determined. This makes the adjustments of the side connections (see fig. 1), leading to the Thomson galvanometer, a matter of considerable ease.

The following pages give some details of the study of the various metals examined.

Gold.

The experiments which furnished the results already published, were made with gold leaf so thin as to be transparent. In order to reduce those results to the form since adopted, it would be necessary to know the thickness of the gold strip. This thickness might be determined roughly, if we knew the the specific resistance of the material and the actual resistance of the strip, which is now destroyed. The latter value is known approximately, and, by assuming the specific resistance to have been that of pure gold, we might arrive at a value of the ratio $\frac{M \times V}{E'}$. This value, however, would be very much larger than that obtained when thicker strips of metal are used, and facts to be hereafter mentioned make it appear quite probable that the thickness of the strip, as above arrived at, is several times smaller than the true thickness.*

Without attempting therefore any accurate determination of the constant of this first strip (A), I pass on to

Gold Leaf, Plate (*B*).

This plate also is of very thin metal, and in general I shall use the term gold *leaf*, when speaking of the metal in this shape, and use the term gold *foil* to denote the strips of considerable thickness.

* See also Albert v. Ettingshausen, "Bestimmung der Absoluten Geschwindigkeit." etc. Sitzungsberichte Akad. Wien, vol. lxxxi, p. 446, 1880. He found the value of the thickness indicated by the weight in similar cases, to be from four to ten times as great as that indicated by the resistance.

This second plate of gold leaf was not constructed until after several thick plates had been tried and found to give very different results from those obtained with the first thin plate in the manner described above. Thinking that some experimental error in the first measurements might account for the discrepancy, and the first plate being destroyed, I constructed the second one. In making observations with this plate I first used the high resistance Thomson galvanometer, whereas the low resistance instrument had been used with the thick plates. Thinking that I might in changing instruments have fallen into some error, I afterward made another series of observations with the same plate, but using the low resistance galvanometer. The results were, the thickness here also being estimated as above described,

March 18, with high resist. galv., $\frac{M \times V}{E'} = 622 \times 10^{10}$

" " " " " $= 637$ .....

" 19, " low " " $= 681$ .....

Mean " $= 647 \times 10^{10}$

This result is about four times as large as those found with thicker plates. Arguing from these facts alone, it would appear that the transverse effect in thin leaf gold is relatively much smaller than the effect in strips of sensible thickness, but this is hardly a safe conclusion. Three objections to the above method of determining the thickness by means of the resistance are evident; 1st, gold leaf so thin as to be transparent is by no means continuous, but is perforated by a multitude of small holes, so that the electricity is, as it were, obliged to wind or zigzag its way through the strip, thereby having a longer path and meeting a greater resistance, than if it could pursue a direct course: 2d, gold leaf is an alloy about twenty-three carats fine, and the resistance of such alloys is often much larger than that of either of the pure metals: 3d, it is difficult to secure good contact at the ends of the strip. In the plate under consideration this contact was probably very bad, and may have been many per cent of the whole resistance of the plate as measured.

All these sources of error affect the result in the same way. To compensate, it would be necessary to diminish the resistance as measured and then, in deducing the thickness, use a specific resistance higher than that belonging to gold. In using thin silver plates I have in a rough way made a correction for the error due to contact resistance, but the gold *leaf* is in several respects so unsuitable for anything like accurate work, that it does not seem worth while to spend any more time upon it at present. In fact I would in the present article dismiss the sub-

ject of gold *leaf* strips with a very few words, were it not the case that, in a matter of this kind, it seems proper that the public should be informed of any facts that have the slightest suspicious appearance.

The gold plates which are now to be described, were of comparatively thick metal, such as is used by dentists. The metal in this shape is said to be very pure, and the thickness was so considerable as to make it possible to weigh the strips with sufficient accuracy. The determination of the thickness in this way involves the assumption that the specific gravity is that given by the tables, but the error from this source must be very much smaller than the sum of those introduced by employing the resistance method.

Gold used by dentists is classed under various heads according to the manner of tempering. The kinds I have used are, I think, "soft," or possibly "semi-cohesive," and "hard," or "cohesive." I noted the varieties, thinking that specific peculiarities might possibly appear in their behavior. The number attached to each plate is the commercial number of the specimen and indicates approximately the number of grains in a sheet about ten cm. square. The letters attached are intended to distinguish different plates constructed from gold of the same number.

Gold Foil, No. 6 (*A*).

This strip was, I believe, of the kind called by dentists "hard," or "cohesive." To determine the thickness it was weighed before being attached to the glass. Previous experiment having shown the great variation in thickness between different parts of a sheet of gold foil, this strip was cut before weighing into nearly the same shape and size that it was to have on the glass.

The strip was in general shape a parallelogram with a projection from the middle of each of its longer sides. The use of these projections, which were much reduced in size before making the observations, has been already explained.

Length of strip when weighed	= 8·50 cm.
Width " "	= 2·14 "
Area including projections	=20·5 " sq.
Weight	= .0848 grms.

Taking the specific gravity of gold at 19·36, the value given by Ganot for "gold stamped," we find

Thickness = ·000214 cm.

With this plate many series of experiments were made, yielding most of the time results which were very discordant, owing to various disturbing causes, some known and others perhaps

unknown, to which allusion has already been made. The results obtained every day, except the last of my working with this plate, are so discordant, that in preparing them for publication it does not seem worth while to go over again the great mass of figures involved, for the purpose of correcting any small errors of calculation. The results obtained were

$$\begin{aligned}
\text{February 20th } \frac{M \times V}{E'} &= 134 \times 10^{10} \\
\text{“ “ “} &= 136 \ldots\ldots \\
\text{“ 23d “} &= 163 \ldots\ldots \\
\text{“ “ “} &= 159 \ldots\ldots \\
\text{“ “ “} &= 166 \ldots\ldots \\
\text{“ 25th “} &= 160 \ldots\ldots \\
\text{“ “ “} &= 149 \ldots\ldots \\
\text{“ “ “} &= 157 \ldots\ldots \\
\text{“ 27th “} &= 152 \ldots\ldots \\
\text{“ “ “} &= 147 \ldots\ldots \\
\text{Mean “} &= \overline{152 \times 10^{10}}
\end{aligned}$$

Replacing now the old perforated poles of the electro-magnet by solid new ones and removing one or two other sources of error, I found

$$\begin{aligned}
\text{March 5th } \frac{M \times V}{E'} &= 150 \times 10^{10} \\
\text{“ “ “} &= 150 \ldots\ldots \\
\text{“ “ “} &= 154 \ldots\ldots \\
\text{Mean “} &= \overline{1513 \times 10^{9}}
\end{aligned}$$

The strength of the magnetic field was, as usual, determined twice on March 5th, once before and once after the other observations. The two values varied by something more than one per cent. The mean of the two is taken as the uniform strength for the day. The strength of the primary current sent through the gold strip was much varied for the different series of observations.

Thus we may write as corresponding to the above three values

Strength of Field. M.	Strength of Primary Current. C.
6400	$k \times \tan 23^\circ\ 44'$
“	“ “ $42^\circ\ 14'$
“	“ “ $49^\circ\ 28'$

when k is the constant of the tangent galvanometer $= \cdot 07$ nearly.

The agreement between the mean of the various results previously obtained and the mean of those found March 5th, was considered satisfactory, and the next measurements were made with

Gold Foil, No. 5.

The metal in this plate was, I believe, either "soft," or "semi-cohesive."

Length of strip when weighed	=	8·49 cm.
Width " "	=	about 3·28 "
Area including projections	=	30·0 " sq.
Weight	=	·1122 grm.
Thickness	=	·000188 cm.

This strip after being placed on the glass was trimmed down to a width of about 2·32 cm., and the mean thickness of this strip was no doubt quite different from the value above obtained. This strip was reduced in width after being weighed more than any other that has been used, and this fact may account for the discrepancy between the results obtained with it and those obtained with the strips of No. 6, already described, and of No. 4, which is to be described next.

With No. 5 were made four series of observations, resulting thus :

	M.	C.	$\frac{M \times V}{E'}$
Mar.	8th, 6400	$k \times \tan 42° 26'$	161×10^{10}
"	" 6330	" " 26° 2′	163 .....
"	10th, 6440	" " 22° 48′	162 .....
"	" 6440	" " 43° 0′	164 .....
		Mean	$= 1625 \times 10^9$

The next plate used was

Gold Foil, No. 4 (*soft*).

Length when weighed	=	7·64 cm.
Width "	=	2·13 "
Area including projections	=	18·46 " sq.
Weight	=	·0478 grm.
Thickness	=	·000134 cm.

With this plate four series of observations were made in one day.

The results obtained March 12th were

M.	C.	$\frac{M \times V}{E'}$
6480	$k \times \tan 22° 21'$	155×10^{10}
"	" " 26° 25′	155 .....
"	" " 42° 16′	154 .....
"	" " 28° 43′	154 .....
	Mean	$= 1545 \times 10^9$

Measurements had now been made with three plates of gold foil, and, considering the irregularity likely to be produced by the impossibility of determining accurately the effective thickness of the strips, the results seemed to agree satisfactorily,

indicating $\frac{M \times V}{E'}$ to be a constant for this metal. If the experiments in gold had begun with these particular plates, they would probably have ended with them for the present. Owing, however, to the great discrepancy observed between these results and those obtained with the very thin plates it seemed desirable to go further, and I therefore constructed a plate using

Gold Foil, No. 30 (A), (*semi-cohesive?*).

Length of strip when weighed	=	5·76 cm.
Width " "	=	1·085 "
Area including projections	=	7·36 " sq.
Weight	=	·161 grm.
∴ Thickness	=	·001129 cm.

With this plate

M.	C.	$\frac{M \times V}{E'}$
Apr. 20th, 6520	$k \times \tan$ 48° 38′	123×10^{10}
" 23d, 6600	" " 31° 30′	124 .....
" " "	" " 46° 39′	128 .....
	Mean	$= 1250 \times 10^9$

This value is about twenty per cent lower than the mean of those obtained with the three plates, Nos. 4, 5, and 6, previously used. The discrepancy was so great, that another plate was made with a strip cut from the same sheet as No. 30 (A).

Gold Foil, No. 30 (B), (*semi-cohesive?*).

Length of strip when weighed	=	5·69 cm.
Width " "	=	1·08 "
Area including projections	=	7.33 " sq.
Weight	=	·149 grm.
∴ Thickness	=	·00105 cm.

It will be seen that the strips (A) and (B), cut from similar positions in the same sheet of metal, differ about seven per cent in mean thickness. The importance of this fact has already been pointed out. The difference in thickness thus found was so great, that I at first supposed a mistake must have been made in weighing the first strip, thereby giving too large a value for the weight. I therefore removed the strip from the glass plate and weighed it again. The result confirmed the original value obtained.

With the new plate, No. 30 (B), I found

M.	C.	$\frac{M \times V}{E'}$
Apr. 26th, 6760	$k \times \tan$ 68° 0	139×10^{10}
" "	" " 39° 26′	141 .....
	Mean	$= 1400 \times 10^9$

This value is much nearer those obtained with the plates 4, 5, and 6, but even now there is a discrepancy of eight or ten per cent. Without discussing this matter any further at present, I pass on to tell what has been observed with

Silver.

Measurements have been made with four separate plates of this metal. The thickness of the strip was estimated in one case by weighing, in the three others by measuring the electrical resistance. I will give first the results obtained with the thick strip.

Silver Foil, No. 10.

Length of strip when weighed	=	7·98 cm.
Width " "	=	1·07 "
Area including projections	=	9·23 " sq.
Weight	=	·0471 grm.
∴ Thickness (taking sp. gr. to be 10·47)	=	·000491 cm.

With this plate

M.	C.	$\frac{M \times V}{E'}$
Apr. 21st, 6580	$k \times \tan$ 49° 17′	114×10^{10}
" "	" " 32° 20′	118 . . . . .
	Mean	$= 1160 \times 10^{9}$

Two of the other plates were prepared, not by fastening silver leaf to glass with shellac, but by depositing from a solution the silver directly upon the glass. The process made use of for this purpose was Böettger's, as detailed in this Journal for 1867. The two plates were cut from the same piece of glass after coating.

Silver Film (A).

Length between the contact blocks	=	6·05 cm.
Width	=	2·46 cm.
Electrical resistance, as measured,	=	1·45 ohms.

Knowing that the contact resistance must be quite a part of this value, I endeavored to determine its amount roughly in the following manner. Having obtained the above value, 1·45 ohms, and measured the distance between the blocks, I shortened the strip by placing the blocks nearer together, then measured the length and again determined the resistance of the whole. This process was repeated, thus giving three values of the resistance corresponding to the three lengths of the strip employed. From these values the contact resistance is readily determined, though of course very roughly. It appeared to be equal to the resistance of about 2·7 cm. of the strip itself, and

therefore in estimating the thickness of the strip from the electrical resistance, the effective length of the strip was taken to be not 6·05 cm., but 8·8 cm. Assuming the specific resistance of the silver in this plate to be ·00000165 ohms, the value given by Jenkin for "hard drawn" silver, we obtain as the thickness of the strip ·00000407 cm. It will be shown below that this value is probably very much too small, but I will for the moment give the results obtained on the basis of this estimation of the thickness.

Passing over a result obtained at quite an early period of the experiments, and which there are excellent reasons for rejecting, we have

M.	C.	$\frac{M \times V}{E'}$
Jan. 30th, 7120	$k \times \tan 43° 33'$	487×10^{10}
" "	" " $19° 32'$	499×10^{10}
	Mean	493×10^{10}

The discrepancy between this result and that obtained with the thicker strip of silver was so great, that I determined to try

Silver Film (B).

I have assumed the thickness of (B) to be the same as that of (A). The other dimensions are about the same, and the result is

M.	C.	$\frac{M \times V}{E'}$
May 4th, 6640	$k \times \tan 47° 39'$	491×10^{10}

The agreement of this result with the mean of those just preceding is entirely satisfactory, and the discrepancy above mentioned, as existing between the results with plates of different kinds, is confirmed. This disagreement was so large as to be difficult to account for, without the hypothesis of a specific difference exhibited by different forms of the same metal, under the conditions of the experiment. To be sure the method of estimating the thickness from the electrical resistance was open to suspicion. Among other probable sources of error there was the possibility of having assumed a wrong value for the specific resistance of the silver in this condition. It did not appear to me probable that an error of about 400 per cent could be accounted for in this way, but it seemed worth while to attempt a determination of the thickness of the films by another method.

Plate (A) was taken and cleaned with alcohol to remove the particles of cement adhering to the glass and metal. The area of the silver film was roughly determined, and the plate was dried and, when cool, carefully weighed. The silver was

then removed by dissolving in nitric acid, after which the glass was again dried and weighed. In addition to this the solution of silver was filtered and treated with hydrochloric acid. The precipitate was filtered off, and the silver reduced by burning with the filter paper. The amount of silver on the glass was thus estimated in two ways. According to the weight lost by the plate the amount of silver appeared to be 4·3 mgr., while the amount obtained by the chemical process was only about 2·5 mgr. There are good reasons for thinking the former value too great and some reasons for thinking the latter too small. Giving the latter double weight in taking the mean we get $\frac{4\cdot3+2\times2\cdot5}{3}=3\cdot1$ mgr. for the amount of silver in the film. The area covered by this on the glass was about 20 sq. cm. Taking the specific gravity of silver to be 10·5, we get for the thickness of the film

$$t=\frac{\cdot0031}{20\times10\cdot5}=\cdot0000148 \text{ cm.}$$

This value is more than 3·6 times as large as that obtained by the resistance method. In order to make perfect accord between the results obtained with the two kinds of silver plates, the thickness would need to be rather more than four times as great as that obtained by the resistance method, but considering all the difficulties of the case, it seems to me that the large discrepancy still existing is within the limits of experimental error. In presenting the results of all the experiments in tabular form further on I shall give the results obtained with these silver films as calculated on the basis of the larger value, i. e., ·0000148 cm., found for the thickness.

Mention is made above of a fourth plate of silver. This was also of a very thin film, but the silver was fastened to the glass with shellac instead of being deposited from a solution. The silver was in the same state as that of the thickest plate, and the results of measurements with it accord sufficiently well with those obtained with that plate. As the resistance method was employed in estimating the thickness, it does not seem worth while to publish the results obtained.

Iron.

Measurements have been made with three separate plates of iron. The first two plates were made early in the research and the quantitative results, like all others obtained at that time, are hardly reliable enough to be worth publishing.

The dimensions of the third strip were as follows:

Length as weighed	= 5·68 cm.
Width "	= 1·08 "
Area including projections	= 7·15 " sq.
Weight	= ·193 grm.
∴ Thickness (taking sp. gr. = 7·79)	= ·00347 cm.*

With this plate the following results were obtained:

M.	C.	$\frac{M \times V}{E'}$†
Apr. 29th, 6680	$k \times \tan 38° 37'$	-127×10^9
" "	" " 49° 13′	−130 . . . .
	Mean	$= -1285 \times 10^8$

Platinum.

One strip of this metal has been used.

Length as weighed	= 6·32 cm.
Width "	= 1·078 "
Area including projections	= 7·57 " sq.
Weight	= ·457 grm.
∴ Thickness (taking sp. gr. = 22·1)	= ·00274 cm.

With this strip only one series of observations was made and that was rather a hasty one; I found

M.	C.	$\frac{M \times V}{E'}$
Apr. 28th, 6830	$k \times \tan 66° 2'$	417×10^{10}

Nickel.

There was some difficulty in obtaining a strip of this metal of proper shape for the experiment. The piece used was obtained by stripping off the nickel plating from a piece of brass, upon which the deposit had been purposely laid in such a manner as to make it easy to remove. The strip thus obtained was narrow and irregular in shape and its thickness cannot readily be determined at present, so that I do not attempt to give numerical results for this metal. The main object in using it was to determine the direction of the new effect therein, nickel being, next to iron and cobalt, the most strongly magnetic substance. As already stated, this direction was found to be oppo-

* The plates of very thin rolled iron used were furnished me by Prof. Rowland, who is indebted for a supply of the same to the courtesy of Prof. Langley of Allegheny Observatory.

† It is evident that the values of this ratio thus obtained for iron are to some extent, perhaps to a great extent, fictitious, for of course the strength of the magnetic field within the iron plate itself is the effective strength in the experiment, and this is probably very different from the value of M as determined by means of the test coil. It seems best, however, for the present, to employ this latter value of M, which must bear an intimate relation to the true value, and which has the great advantage of being easily determinable. Nickel has hardly been examined quantitatively as yet, and platinum is not sufficiently magnetic to present any difficulty of this sort.

site to that in iron. The action in nickel, though not really measured, was seen to be very decided, and may possibly prove to be as strong as that in iron.

Tin.

The action in this metal is very small and has not been measured with any accuracy. Its magnitude may be $\frac{1}{30}$ that of the action in gold.

No other conductors have been tested in such a manner as to warrant an expectation of detecting an action.

In the following table the results obtained with the different metals are brought together. Those obtained with very thin strips will be marked thus (?) for reasons which must be evident to any one who has read the preceding pages:

Metal Plate.	M.	C.	$\frac{M \times V}{E'}$	
Gold, No. 6 ["hard"]			152×10^{10}	
" " "	6400	$k \times \tan$ 23° 44′	150×10^{10}	1515×10^9
" "	6400	" " 42° 14′	150×10^{10}	
" "	6400	" " 49° 28′	154×10^{10}	
" No. 5 [soft or semi-cohes.]	6400	" " 42° 26′	161×10^{10}	
" "	6330	" " 26° 2′	163×10^{10}	1625×10^9
" "	6440	" " 22° 48′	162×10^{10}	
" "	6440	" " 43° 0′	164×10^{10}	
" No. 4 ["soft"]	6480	" " 22° 21′	155×10^{10}	
" "	6480	" " 26° 25′	155×10^{10}	1545×10^9
" "	6480	" " 42° 16′	154×10^{10}	
" "	6480	" " 28° 43′	154×10^{10}	
" No. 30 (A) [semi-cohes. ?]	6520	" " 48° 38′	123×10^{10}	
" " "	6600	" " 31° 30′	124×10^{10}	1250×10^9
" " "	6600	" " 40° 39′	128×10^{10}	
" " (B) [semi-cohes. ?]	6760	" " 68° 0′	139×10^{10}	1400×10^9
" " "	6760	" " 39° 26′	141×10^{10}	
Silver, No. 10	6580	" " 49° 17′	114×10^{10}	1160×10^9
" "	6580	" " 32° 20′	118×10^{10}	
" [deposited] (A)	7120	" " 43° 33′	134×10^{10}	1355×10^9 ?
" " "	7120	" " 19° 32′	137×10^{10}	
" " (B)	6640	" " 47° 39′		1350×10^9 ?
Iron (C)	6680	" " 38° 37′	-127×10^9	1285×10^8
" "	6680	" " 46° 13′	-130	
Platinum	6830	" " 66° 2′		4170×10^9

Nickel—effect large, possibly as strong as in iron.
Tin—effect probably much smaller than in platinum.

This table enables us to arrange the metals so far examined, excepting nickel, in order with respect to the magnitude of the action observed in them. Opposite each metal in the following list is placed a number representative of this magnitude. In the case of gold this number is a quantity inversely proportional to the mean of the results obtained with the five different plates named above. In finding the corresponding number for silver, I have, for obvious reasons, used only the result obtained with the plate of No. 10. The representative number given for tin has been very roughly estimated and may be one

or two hundred per cent larger or smaller than the true number. All the numbers given must of course be taken as at best only rough approximations to the true representative numbers.

We find then

Iron	— 78·	Platinum	2·4
Silver	8·6	Tin	·2 (?)
Gold	6·8		

This arrangement is made on the basis of defining the magnitude of the action studied as a quantity inversely proportional to $\frac{M \times V}{E'}$. If on the other hand we were to define the same as inversely proportional to $\frac{M \times E}{E'}$, rather, E being the difference of potential of two points a centimeter apart on the longitudinal axis of the metal strip, the representative numbers would be relatively changed. The representative numbers on this new basis may be found by simply dividing each of the representative numbers given above by a quantity proportional to the specific electrical resistance of the metal to which the number is attached.

We thus obtain

Iron	— 80·	Platinum	2·6
Silver	57·	Tin	·15 (?)
Gold	32·		

It will be observed that the order of arrangement remains unchanged.

Platinum and tin are carried still farther from gold and silver than before, so that the range of the representative numbers is increased. It is plain, therefore, that by this second arrangement no progress has been made toward finding a constant representative quantity for all the metals. In dealing with the results obtained with different metals, it seems to be of little importance whether we take as our basis $\frac{M \times V}{E'}$ or $\frac{M \times E}{E'}$. When, however, we have to do with different plates of the same metal, we see from the experiments on both gold and silver that the basis $\frac{M \times V}{E'}$ is by far the better one. We may sum up the matter by saying that according to present appearances:

1st, there is no constant representative quantity for all metals;

2nd, the basis $\frac{M \times E}{E'}$ does not give a constant representative quantity for different plates of the same metal;

3rd, the basis $\frac{M \times V}{E'}$ gives for different plates of the same metal a representative quantity which is approximately a constant.

It is evident, upon consideration, that this ratio $\frac{M \times V}{E'}$ could not be expected to give the same result for all metals. We get the quantity V by dividing the nominal cross section of our conductor by the strength of the current. We must, however, think of a metal as not strictly continuous, but consisting of metallic particles more or less compactly aggregated in the space occupied by the body as a whole. Evidently, therefore, the cross-section effective in conduction would vary in different conductors of the same nominal cross-section. It may, therefore, be found that different specimens, of the same metal but of different densities, will give quite different values for $\frac{M \times V}{E'}$.

Of course the magnitude of the new action in the different metals may be considered in connection with various other physical properties of the metal beside the specific electrical resistance. One might for instance expect to find some striking relation by comparing in this connection the known magnetic or diamagnetic properties of the metals. It is indeed to be observed that the most strongly magnetic substance, iron, does show the new action in a more marked degree than the other metals, and possibly nickel will come next in the list. Here the clue is entirely lost however, for the relative magnitude of the action in gold, silver, etc., is entirely out of proportion to the magnetic capacities of these metals.

On the whole we cannot be sure that any relation has yet been detected between the magnitude of the new action in the various metals and any known physical property of these metals. It is of course possible, however, that when more data shall have been obtained, analogies and relations at present unsuspected will appear. It can hardly be doubted that the action we have been considering, placing at our command, as it does, a new point of view from which to study the interior workings of the substances examined, is destined to teach us a good deal in regard to the molecular structure of bodies, while helping us toward an understanding of the physical nature of electricity and magnetism.

We return now to the remarkable anomaly presented by the direction of the action in iron. That the direction in this metal, a magnetic substance, should be different from that in gold, a diamagnetic substance, is remarkable, but not perhaps surprising. We find, however, that nickel and platinum, both magnetic substances, resemble in the particular above mentioned, not iron, but gold, and the other diamagnetic substances. This fact has to be taken into account in endeavoring to apply the newly discovered action to explain the magnetic rotation of the plane of polarization in accordance with

the principles of Maxwell's electro-magnetic theory of light. Professor Rowland, therefore, in view of this difference of behavior of iron and nickel with respect to electricity, was very desirous to know whether these two metals would manifest a similar disagreement in their action upon light. I have, therefore, at his suggestion, repeated Kerr's experiment on the rotation of the plane of polarization of light by reflection from the pole of a magnet, using nickel for the latter instead of iron. The reflecting surface used was the nickel plating on one of the disks of Professor Rowland's absolute electrometer. This disk for the purpose of the experiment was placed between the poles of the electro-magnet. The action upon the plane of polarization, though apparently much weaker than in iron, has, in the plate used, unmistakably the same direction. This nickel plating, however, was executed in Germany, and Professor Rowland thinks that, as the nickel of that country is very impure, this specimen may possibly contain iron enough to mask the true action of the nickel.

I have already spoken of the fact that, when a strongly magnetic substance is experimented upon, complications are introduced by the influence of the induced magnetism which affects the condition of the magnetic field through which the current flows, making the value of M different from that determined by means of the test coil. It does not seem probable that in this fact can be found an explanation of the anomalous behavior of iron, but there is no doubt that an interesting research is here suggested. For instance, it might be profitable to subject to experiment a thin plate of hard steel and determine to what extent the permanent magnetization induced therein by the electro-magnet would be accompanied by a permanent change in the equipotential lines after the electro-magnet had ceased to act.

It is perhaps idle to speculate as to the exact manner in which the action between the magnet and the current takes place in any of the preceding experiments, but it may be worth while to remark a seeming analogy, somewhat strained perhaps, between this action and a familiar mechanical phenomenon, the theory of which has of late attracted considerable attention. It is well known that a base ball projected swiftly through the air and having at the same time a rapid motion of rotation about its vertical axis does not throughout its course continue in its original vertical plane of its motion, but follows a path curving sensibly to one side. Imagine now an electrical current to consist of particles analogous to the base ball, moving through a metallic conductor the electrical resistance of which will correspond to the mechanical resistance offered by the air. Suppose, further, the particles of elec-

tricity, on coming within the influence of the magnet, to acquire a motion of rotation about an axis parallel to the axis of the magnet.* Under all these supposed conditions we might perhaps expect to find the action which is actually detected. To account for the reversal of the action in iron, we might suppose the particles of electricity to acquire in this metal a rotation about the same axis as in the other metals, but in the opposite direction. Even after all these generous concessions in favor of our hypothesis, however, it fails to account for the behavior of nickel, as different from that of iron. The analogy, such as it is, which has been pointed out, is perhaps curious rather than significant.

Historical.

I am not aware that investigators, during the first part of the century, made any attempt to discover the phenomenon which has been the subject of the observations described in the preceding article. Wiedemann,† however, mentions two investigators who have at different times given the subject their attention. The first of these in point of time was Feilitzsch.‡ He made use of two flat spirals of wire, through each of which an electric current was made to pass. These currents, passing in opposite directions through the coils of a differential galvanometer, were so adjusted that their combined action produced no effect upon the needle. A third spiral, similar to the others and itself bearing a current, was now brought near one of these and the galvanometer was observed. No permanent deflection of the needle was detected, and therefore no permanent action of one current on the other was discovered. I have not had access to the original article and cannot say what the author's theory of the experiment may have been. The method of attacking the problem seems however to have been similar in principle, to that which I at first adopted, viz: an endeavor to increase the resistance experienced by an electrical current by diverting it from its normal course through the conductor.

Another research in this direction mentioned by Wiedemann was that of Mach.§ This investigator covered a circular disk of silver leaf with wax and applied the poles of a battery to points diametrically opposite each other on the circumference of the disk. The silver leaf becoming heated by the cur-

* Maxwell (Electricity and Magnetism, vol. ii, p. 416) says, "I think we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field, that this rotation is performed by a great number of very small portions of matter, each rotating on its own axis, this axis being parallel to the direction of the magnetic force," etc.

† Galvanismus, vol. ii, p. 174.

‡ Berichte der Naturforscher in Karlsruhe, 1858, p. 151, etc.

§ Carl's Repertorium, vol. vi, p. 10, 1870.

rent, the wax began to melt and melted most rapidly where the current was strongest, thus roughly showing the distribution of the stream. The plate still bearing the current was now subjected to the action of an electro-magnet, but no change could be detected in the behavior of the melting wax, the current remaining apparently unchanged in its course through the disk. This experiment therefore, like the preceding, was negative in its indications.

A recent number of the "Beiblätter zu Wiedemann's Annalen" mentions, in connection with the researches of Feilitzch and Mach, another by Gore.* The latter took a wire bifurcated throughout a part of its length and passed through it a current sufficiently strong to raise both branches to a white heat. He then endeavored by means of a magnet to divert the current somewhat from one branch of the wire and draw into the other branch more than its normal share. It was thought that an unequal division of the current might show itself by a change in the appearance of the white hot branches. No change of this kind could be detected, and the investigator therefore concluded that the action known to take place between conductors bearing currents, was not an action between the electric currents as such. Gore expressly states that he undertook this experiment not knowing that any previous investigations with the same aim had ever been made.

On the same page of the "Galvanismus" which treats of the research of Mach, as mentioned above, Wiedemann describes, as a means of showing that no action takes place between permanent electric currents as such, almost the exact arrangement of apparatus with which the discovery was finally made. Who first used this apparatus for this purpose I cannot say, unless it may have been Wiedemann himself. The same plan was hit upon by Professor Rowland,† quite independently I believe, and he experimented to some extent in this direction about the year 1876. The same arrangement was finally adopted by me after another method of attacking the problem had been unsuccessfully tried.

I desire to express my sense of obligation to the professors and students of the physical department of the Johns Hopkins University, for the generous assistance which they have rendered me during the progress of this research.

* "On the Attraction of Magnets and Electric Currents."—Phil. Mag. (4th series), vol. 48, p. 393, 1874.

† Amer. Jour. of Math., vol. ii, p. 289.

ART. XXIII. — *The Colors of Thin Blowpipe Deposits;* by C. H. KOYL, B.A., Student of Physics in Johns Hopkins University.

SOME examples of the action of very fine particles of matter upon light, having lately come to my notice, it may be interesting to make them public, as they have heretofore, I believe, been unexplained.

Those who are familiar with the methods of blowpipe analysis have observed faint borders occasionally surrounding some of the colored charcoal coatings, the colors of these borders seemingly bearing no relation to the characteristic colors of the adjoining oxides. For instance, the white coating of antimony is generally accompanied with a blue border, the brownish oxide of cadmium occasionally with a green, while the lead and bismuth yellows not unfrequently have a whitish ring inclosing them. As these occur only and always where the coating is very thin they have a significance different from that of the ordinary colors, and as they may be produced at pleasure from the purest specimens they cannot be due to mixtures of the metals. A possible analogy with the antimony blue was suggested by a consideration of the colors of the sky, and to prove the connection it was simply necessary to show the similarity of attendant phenomena. As is well known, it is believed that the blue of the sky is due to the presence in the atmosphere of suspended particles, so fine that they are unable to reflect the longer rays of the spectrum which accordingly are transmitted and the union of the remainder gives to the sky its blueness. At evening, the sky is red because we get the rays of the sun directly transmitted or reflected from the clouds. Thirdly, the light of the sky, reflected at an angle of 90° with the sun, is plane polarized.

When an antimony coating had been produced which gave, beyond the white oxide, a blue well defined and full, the whole was illuminated in a dark room by a sodium flame and that the blueness was no psychical or physiological effect as distinguished from ordinary vision was proved by the fact that here it almost completely vanished while the white presented the usual ghastly appearance. A blue book-cover, treated in the same manner, gave more reflection than did the blue coating.

Experiments with the polariscope were at first inconclusive from the fact that though the light from the blue coating was largely polarized, so, to some extent, was also that irregularly reflected from the charcoal, and it was found necessary to cover the block with a thin layer of carbon from a gas-flame. The

repetition of the test then showed that the proportion of light polarized by the layer of carbon, at the given angle, was almost nothing; that by the thick white coating, small; while on the blue the phenomenon was almost complete. What light here was not polarized was evidently reflected from the larger particles mixed with the fine, for the analyzer, while it did not totally extinguish the light, yet excluded nearly all appearance of blueness.

In order to determine the character of the transmitted light, a microscope covering-glass was inlaid in the charcoal and the oxidation so executed that the glass was in the center of a small area all of which was blue. On removing the glass, the light which passed through, proved to be of the expected yellow, though less brilliant than anticipated. The color might be seen either by transmitting the direct light of the sun or by placing the glass at such an angle that total reflection was produced and thus in the passage of the rays through the layer to the glass and out through the layer to the eye the blue was principally lost and only the mixture of longer rays appeared. Viewed through a microscope, the result was the same. I have since, however, improved upon this plan by the more convenient method of covering with carbon a piece of ordinary window-glass, three inches by two, and then projecting the oxide upon the *opposite* surface of the plate. There is thus no difficulty in distinguishing a very slight amount of color in the coating and for transmitted light, any portion of the carbon may be easily removed.

This case, a type of all charcoal coatings which shade off to blue in thin layers, appears thus parallel to that of the sky color and the theory which is accepted for the one will also satisfactorily explain the other.

To account for the cadmium green we have only to note that if the substance upon which we are experimenting have the power of absorbing the shorter rays of the spectrum, the reflected light would from a heavy coating be yellowish or reddish, the particular shade depending upon the amount of absorption of violet and blue; and the formation of a layer as thin and of particles as fine as before should result in giving us the color of the shortest rays which the substance is capable of reflecting, viz: in this case, green. The coating of cadmium has exactly this appearance and shows the effect of the gradual transmission of red by shading from the original color (dark red) through yellow into a fine green. As before, the light reflected from the thin layers is highly polarized and the rays which pass through form a deep, dark red. In exceptional cases, it is possible to produce such a thin coating that the extreme edge is fringed with a faint blue.

The other case, lead, is now easily explained. This metal gives a coating of which the color is a beautiful chrome yellow, and regarding this merely as a repetition of the preceding phenomenon and the yellow as compounded of rays from the whole range of the spectrum but not in the proper proportion to form white, the line of thought suggested evidently is, that if the layer be decreased in thickness regularly from the center to the circumference of the charcoal, there ought to be, at some distance from the centre, a zone within which sufficient red should be transmitted to equalize the amount of blue lost by absorption and the reflected rays should form a yellowish white. Beyond this, as the thickness of layer still decreased, the color should be blue for the same reason as in the case of antimony. The white zone is easily produced and the blue border, which always surrounds it, polarizes the light as before and transmits orange colored rays.

The theory, once given, serves to explain nearly all the anomalous colorings of the charcoal coatings;—the bluish borders which occasionally skirt almost any of the metallic oxides, the "peacock-tails" of cadmium, etc., and thus does away with the necessity of supposing the presence of impurities (though, by the way, no impurity would solve the problem in the case of the cadmium green.)

From a physical standpoint, the experiments seem interesting as an extension of our knowledge of the action of these small particles upon light. Had not the subject presented itself in this way, we would scarcely have guessed that such a change in reflecting power could have been produced by so small a change in size and thickness.

Baltimore, Md., July 9, 1880.

ART. XXIV.—*The Periodic Character of Voluntary Nervous Action;* by M. M. GARVER.

IN the June number of this Journal for 1878 (No. 90, vol. xv, pp. 413–422), in an article on Nervous Transmission, I called attention to a peculiar grouping of the answers in experiments requiring a voluntary movement. In concluding that article I suggested an explanation of the grouping by intimating that an individual had to "think twice" before performing the stipulated action, and ended with the following query and statement: "Could not such a periodicity have its origin in the transformation lying between sensation and volition? It is easily conceivable that such might be the case and be of such a nature as to resemble an increment to the judgment."

Further study of the subject has led me to the conclusion that the expression "think twice" is *literally* true, and that the variation *is* entirely cerebral." The matter can probably be best presented in the form of an hypothesis, supporting it by what proofs may be at hand. The hypothesis is this:

The cerebral portion of the nervous system is continually varying in its activity, waxing and waning between certain limits, periods of maximum activity following periods of minimum activity at the rate of 36 to 60 times per second.

The first proof in support of this view is that it offers a satisfactory explanation of the grouping in those experiments to which attention was called in the preceding article. For, if such a condition of things exists it is evident that the purely voluntary movements must also be periodic in character—it being impossible, according to the hypothesis, for a voluntary impulse to originate during a period of minimum activity or rest. The fact that the periods are not more sharply defined is not conclusive proof of the non-existence of such cerebral variations, for there is evidence to show that the periods are somewhat variable in different individuals, and in the same individual under different circumstances. For instance, it is generally conceded that the brain does not work so well after a hearty meal as before. Here are two series of experiments, one of which was taken immediately before dinner, the other soon after.

Garver, "hand to hand."

BEFORE DINNER.

16	17	18	19	20	21	22	23 vib.
16	17	18	19	20	21	22	
16	17	18	19	20	21		
16	17	18	19		21		
	17		19		21		
	17		19		21		
			19		21		

The numbers below the line are the answers to the signals and show the time expressed in vibrations of the tuning-fork, —one vibration being equal to $\frac{1}{128}$ of a second. An evident preference is shown for the numbers 17, 19, 21.

Garver, "hand to hand."

AFTER DINNER.

16	17	18	19	20	21	22	23	24	25	26	27	28	29 vib.
16	17	18	19	20	21	0	23	24	25	26	0	0	29
	17	18	19	20	21		23			26			29
	17	18	19	20			23						
		18	19	20			23						
		18	19	20									
		18	19	20									
			19										
			19										

Besides these numbers there were many failures, in the last series, to answer at all. This last series differs in one respect from many others. The periods are not all of the same length; and if this should prove to be a rule and not an exception a modification of the hypothesis would be necessary. At present I am inclined to regard it as accidental, as it is opposed to so many well-defined examples. The periods after dinner are seen to be lengthened and the whole series drawn out.

That the grouping is not due to the nerves themselves is shown by the fact that the nerves of animals recently killed transmit the motor impulse with perfect regularity; and also by the fact that, in the living subject, nerves excited by artificial means transmit the motor impulse with the same regularity.

2d. The muscles, in order to remain in sound health, must have periods of rest alternating with periods of activity. Even the heart, that keeps the blood in ceaseless motion from the earliest dawn of our independent existence till the last closing act in life's drama, rests about one-half the entire time; and it certainly appears reasonable to suppose that the brain also has its periods of rest. Besides this, it is an established physiological fact that a muscle during contraction is in a state of vibration, giving out a continuous sound like a musical tone. According to Helmholtz* the pitch of the fundamental tone is very difficult to determine, because the rate is such that it lies just at the lower limit of continuous tones. The rate of vibration varies somewhat, but Helmholtz and two other observers working independently agree in finding thirty-six vibrations per second as the rate, although Helmholtz himself found, in some cases, thirty-two to be the number. Foster† states that the sound heard indicates thirty-six to forty vibrations, but that the sound is the first harmonic, and that the real number of vibrations is one-half that number, or eighteen to twenty per second. The period found in our experiments is from thirty-six to sixty. If the muscular system is subject to such pulsations it seems reasonable to suppose that the nervous system is subject to a similar law, if not really the cause of the muscular variations.

3d. All of the simple mental or psychological processes, the time of which has been measured (and many such measurements have been made), require a *longer* interval of time than that shown by experiment to pass between two maxima and minima. This fact is regarded as specially significant, for, if such waxing and waning of nervous activity exists, the sim-

* Helmholtz, Ueber das Muskelgeräusch; Reichert u. du Bois Raymond's Archiv für Anatomie, 1864, p. 766.

† Foster, Text-book of Physiology, 2d edition, p. 56.

plest possible element of thought would probably require at least one period of maximum activity, while more complex processes would require two, three, or some higher multiple of that number. According to experiments by Burckhardt,* Professor Donders, and many others, the time required by an intelligent person to perceive and to will is about $\frac{1}{10}$ of a second. To illustrate, take an example from Burckhardt.* After allowing for the time required to traverse all of the nerves and for the latent period of the muscles, there still remains about $\frac{1}{10}$ of a second for the cerebral operations. When the signal was given by a bell and the answer required a movement of the hand, the percentage of the time required for the different operations was found to be as follows:

Acoustic nerves	6 per cent.
Brain	62 "
Spinal cord	4 "
Nervous transmission	22 "
Latent period of muscles	6 "

The mean value for the time required from "ear to hand" was 0·169″, of which 0·105″ or sixty-two per cent was taken up by the mental operation involved. From this it will be seen that "quick as thought" is after all not so very quick. Similar results were obtained by Exner.† In our experiments (see this Journal for June, 1878, p. 416), the "reaction period" from ear to hand varied from 0·1327″ to 0·1651″. The latter number is my own "reaction period;" and if sixty per cent of the time was consumed in the cerebral operation it takes $\frac{1}{10}$ of a second for me to perceive and will.

The foregoing results were obtained by answering to an expected and known signal; however, if a dilemma is introduced, offering a choice, the time required is considerably lengthened, and the lengthening is greater as the mental operations are more complex. Professor Donders‡ made some experiments in which the answer was required by the left hand when the signal was given on the right foot, and by the right hand when the signal was given on the left foot. After allowing 0·009 of a second for the less ready use of the left hand, the results agreed exactly in requiring $\frac{1}{10}$ of a second for the increased psychological processes involved. That is, it took $\frac{1}{10}$ of a second longer than if the signal was always given in the same place and required the same answer. By combining three series of observations of different degrees of complexity, Professor Donders found in his own case that the simple percep-

* Burckhardt: Die physiologische Diagnostik der Nervenkrankheiten, Leipzig, 1875, p. 91.

† See Foster's Physiology, p. 522.

‡ Donders: Die Schnelligkeit psychischer Processe; Reichert und du Bois Raymond's Archiv, 1868, p. 657.

tion of an impression required $\frac{1}{2}\frac{1}{5}$th, and an act of volition $\frac{1}{2}\frac{1}{8}$th of a second. The sum of the two is practically equal to what has been found to be the time required for the cerebral operations in simply answering a signal! But each of these periods is long enough to include *two* periods of maximum activity; and this fact might lead us to infer that the so-called simple operations, perception and volition, are in reality complex, and that the simplest mental operation requires about $\frac{1}{5}\frac{1}{0}$th of a second. However, the fact that from $\frac{1}{2}\frac{1}{8}$th to $\frac{1}{3}\frac{1}{6}$th of a second is necessary for a simple perception may serve to explain our appreciation of continuous sounds, as a musical tone; if the vibrations occur at a less rapid rate than that they are recognized as separate sounds,—if at a higher rate as continuous tones.

4th. It is maintained by some writers upon purely metaphysical grounds that alternating states of consciousness and unconsciousness are necessary to explain some of the phenomena of mind. Dr. Payton Spence* advances some views which in this connection are very interesting. He says in conclusion: "Hence the simplest form of consciousness or mental life must consist in an alternation of a state of consciousness with a state of unconsciousness—a regular rhythmical revelation of the affirmation, consciousness, by its negation unconsciousness, and *vice versa*. We might call it a pulsation or an undulation of the constituent of the mind, provided such an expression did not fasten upon us a premature theory as to the nature of that constituent. Perhaps it would be safer, for the present, to call it a pulsation or an undulation in the brain, or a vibration of the molecules of the brain paralleled in consciousness. This pulsation or vibration is, of course, very rapid; otherwise, we should not have to infer its existence, but would know it by perceiving the alternation of one state with another. We may make it to some extent perceptible, however, by interfering with the regularity of its rhythm, as by making a determined, persistent effort to retain any state of consciousness for any length of time. Thus, if we fix the eye upon any object and try to keep a steady, unbroken consciousness of it, we will find that in spite of our most determined efforts, the mind will alternately flash off and on the object, and we catch ourselves losing our consciousness of it and then returning to it. If the experiment be persevered in it ultimates in a certain bewilderment and confusion of mind as well as of vision, and during brief intervals not only does the object cease to be visible, but the mind seems to go out."

Dr. Spence may object to my "hypothesis" as a "premature theory;" however, his words express quite clearly my views, and seem to accord well with the facts in the case.

Ithaca, N. Y., July 1st, 1880.

* Dr. Payton Spence: Space and Time considered as Negations, Journal of Speculative Philosophy for October, 1879.

ART. XXV.—*Geological Relations of the Limestone Belts of Westchester County, New York;* by JAMES D. DANA.

[Continued from page 32.]

(4.) *Hornblendic, Augitic, and other associated Rocks not included in the preceding subdivisions.*

THE hornblendic and associated rocks referred to in the above title cover a large part of the township of Cortland—the northwestern of Westchester County — between Croton River on the south, and the parallel of Peekskill on the north, an area of about 25 square miles. They differ widely from the ordinary rocks of the county, and may well be designated the Cortland series. In fact, a series so remarkable in constitution, so diversified in kinds and so full of geological interest is seldom found together within so small an area any where on the globe. They reach the banks of the Hudson just south of the Peekskill railroad station, and at several points beyond; yet considerable portions of the shore region are occupied by narrow strips of common kinds of mica schist and gneiss, and occasionally limestone. Leaving Peekskill by South street, near the river, the first ledges (north and south of Hudson street, *b*, on the following map), consist of one of the rocks of the series; and to the eastward of the village, on the road leading southeast, only half a mile from the Academy grounds (adjoining which, on the street north, an evenly bedded mica schist of the limestone series outcrops), the same rocks occur. The western boundary of the town is passed by the Cortland rocks only south of its middle point (below Mr. Strang's), for a distance of a little over a mile.

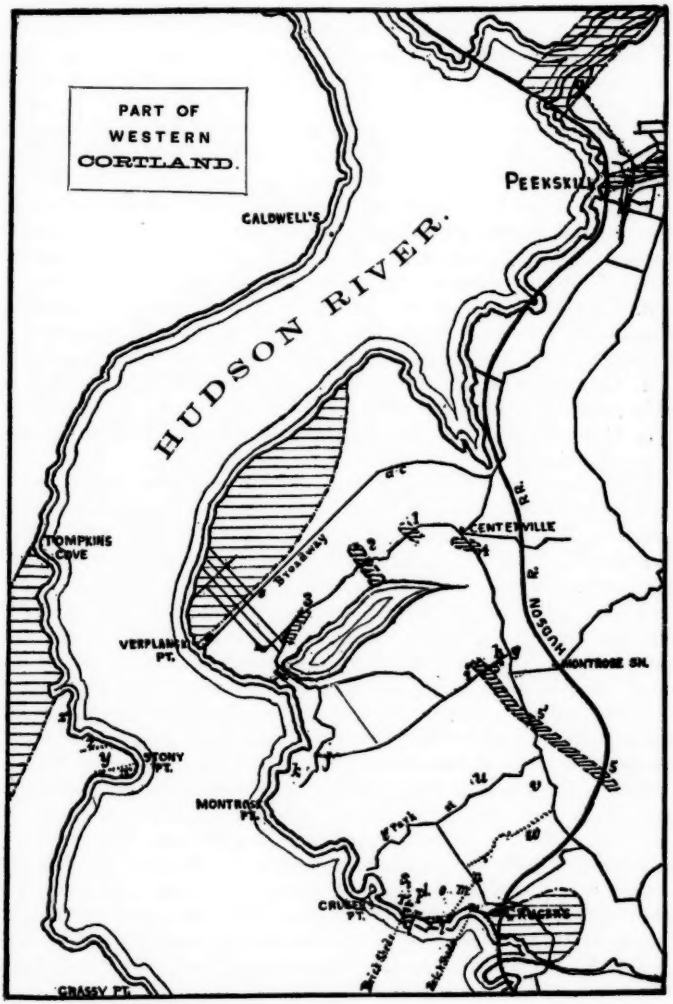
South of Verplanck they extend to the Hudson, and are the rocks of Montrose Point and the northern portion of Cruger's Point. Just here the river becomes narrowed to one third of its width through the projection of these points and of an equally prominent headland called Stony Point on the opposite side. This isolated east-and-west ridge consists of rocks related to those of Montrose and Cruger's Points, and there is little doubt that it was once connected with the Montrose region. It is the only locality of the rocks yet observed on the western side of the Hudson.

The accompanying map of the western portion of the town of Cortland, between Peekskill and Cruger's, together with the Hudson River adjoining, contains the places here referred to. Its scale is an inch to a mile.

The occurrence of limestone areas in close proximity to the rocks of the Cortland series is a fact of special interest, as is

shown beyond. These areas on the maps are those horizontally lined.

2.



A brief description of the prominent varieties or kinds of these Cortland rocks will prepare the way for a discussion of their relation to the other rocks of Westchester County.

A. KINDS OF ROCKS.

The more prominent peculiarities in the constitution of these rocks, (as learned by the aid of thin slices and microscopic examination,) are as follows:

1. The feldspars are chiefly triclinic species, or soda-lime feldspars, though some orthoclase (potash-feldspar) is also often present. They are therefore distinctively what many would call "*plagioclase*" rocks.

2. One or more of the minerals of the Amphibole group —hornblende, hypersthene, augite—are present in a large part of the rocks; of these, hypersthene is the most widely distributed. Its crystals, which are sometimes quite perfect, have the form of the augite common in volcanic rocks except that they are not oblique; they were ascertained to be true hypersthene through optical methods by Dr. G. W. Hawes.

3. Black mica or biotite is usually present, and sometimes abundantly, and in some of the kinds replaces wholly, or nearly so, the iron-bearing amphibole minerals.

4. Quartz is not a prominent ingredient, and in general is only sparingly present.

5. Chrysolite is a characteristic ingredient of some of the common kinds.

6. Apatite exists in microscopic and sometimes visible crystals in all the varieties; the largest crystal observed has a length of half an inch and diameter of a sixteenth. Magnetite is present in grains, and sometimes constitutes beds. Pyrite also is disseminated through most of the rocks.

These crystalline rocks are commonly massive, that is, without bedding. They are everywhere jointed, and for this reason the ledges are generally piles of large and small blocks. In most places they undergo easy decomposition, making a gray or iron-red soil around; and, as the joints give access to water, the outer blocks in the pile have often become reduced to rounded and half-detached masses.

The rocks may be divided, for the convenience of the stratigraphic discussion beyond, into (1) the *non-chrysolitic*, and (2) the *chrysolitic*. The former include four groups, based on the iron-bearing silicate prominent in the kinds; (A) the *Hornblendic;* (B) the *Hypersthenic;* (C) the *Augitic;* and (D) the *Micaceous* or *Biotitic;* but the groups pass into one another by intermediate varieties. The chrysolite-bearing kinds are either (E) *hornblendic*, or (F) *augitic*, or (G) chiefly chrysolite; but here again intermediate kinds occur.

In the following descriptions I have confined myself to noting only the prominent distinctions so far as necessary to the stratigraphical discussion beyond. I take pleasure in stating that a detailed study of the rocks of the Cortland region has

already been begun, at my suggestion, by the accomplished lithologist, Dr. G. W. Hawes.

A. *The Hornblendic.*—The common hornblendic rock resembles syenyte, but contains little orthoclase and much triclinic feldspar. The latter is mostly of the species oligoclase, according to an optical measurement on cleavage slices. In addition, quartz is rather sparingly present. The rock contains more or less black mica and sometimes much of it; and as the mica increases at the expense of the hornblende, the rock passes into soda-granite (mentioned beyond). The quartz-dioryte has the same relation to soda-granite that quartz-syenyte has to potash or common granite. Garnets are rare. The rock is often a very coarsely crystallized rock (*Aa*), having the hornblende crystals large, one-fourth to one-half an inch being a common size, and an inch and larger also common; and not unfrequently the black crystals are as large as the fingers, and occasionally six to eight inches long. A fine-grained variety (*Ab*) has a blackish color; and this variety is sometimes porphyritic (*Ac*). The micaceous is another common variety (*Ad*). Another kind (*Ae*) is exceedingly fine-grained and consists of minute grains of hornblende along with similar feldspar grains, which are partly orthoclase; it looks much like hornblende schist, and in some places is schistose. It sometimes contains an occasional crystal of hypersthene. Another rock of the region is hornblendyte, consisting almost wholly of hornblende. One variety (*Af*) is made up of coarsely crystallized black hornblende; another (*Ag*) of gray hornblende along with an asbestiform mineral. The black hornblendyte graduates into dioryte; and again, it is often chrysolitic.

B. *The Hypersthenic.*—The typical rock of this division (*Ba*) is the most wide-spread of the Cortland series. It consists of triclinic feldspars (and, according to some trials, yet incomplete, by Dr. Hawes, oligoclase is the most abundant), hypersthene in grains or quite small crystals, with frequently more or less biotite, and often some orthoclase. There are also present some magnetite and apatite, frequently traces of quartz, and generally some augite or hornblende. In mineral constitution it approaches one of the kinds of rock that have been called both noryte and hypersthenyte or hyperyte. The name *noryte* is here given it provisionally. It, however, looks more like a coarsish doleryte or diabase than like other hypersthene rocks.

The noryte has commonly a dingy, brownish-red color on a surface of fracture owing to the smoky-red color of the feldspar, but varies from this to pale gray on one side and blackish-gray on the other. It occurs along the railroad between Peekskill and Montrose station, and over the most of the town of Cortland east of this line, and to some extent west.

This rock passes into a feldspathic kind (*Bb*), consisting almost wholly of the feldspars; and into a micaceous kind (*Bc*) containing very much black mica with little hypersthene —a very common variety, often occurring close alongside of the ordinary noryte.

Although the noryte is generally a massive rock, it is occasionally distinctly gneissic in structure, and sometimes contains a few garnets. The schistose variety usually abounds in black mica, or contains more quartz than other varieties, and sometimes more orthoclase.

C. *The Augitic.*—True augitic rocks are less common than hypersthene rocks, although augite is present in most of the massive rocks of the Cortland region. The chief kind (*Ca*) is pyroxenyte—consisting mainly of pyroxene or augite, with sometimes a little hornblende; it varies from a very coarse rock with the augite crystals half an inch across, to a fine granular kind. A greenish-gray granular variety occurs on Stony Point in its chrysolitic region. Another kind (*Cb*) contains much triclinic feldspar with the augite, and is here called *augite-noryte.*[10] A local variety (*Cc*), related to the last, is light gray in color and smooth in fracture; it has a whitish feldspathic base, seemingly almost felsitic, speckled with small spots or points of dark gray-green augite, and only traces of mica. The feldspar in this variety, as slices show, is actually in fine crystalline grains; almost all of it is triclinic, as in other varieties. Chrysolitic kinds are mentioned beyond.

D. *The Micaceous.*—The micaceous rocks are of two prominent kinds. One (*Da*) is like a coarse granite in aspect; but its feldspathic portion is chiefly triclinic, and quartz is sparingly present. It is characteristically a soda-lime granite, although containing some orthoclase, and it is a nearly quartzless variety of it. The mica is almost solely biotite or black mica. It is called beyond *soda-granite.* Some hornblende or augite is usually present; and apatite is common in small or minute disseminated crystals. It is sometimes sparingly garnetiferous. Good examples of this rock occur west of Cruger's railroad station above the brick yards; and also at Stony Point, where it is the prominent rock. At the former locality it graduates into the quartz-dioryte; and at several places the coarsest of the dioryte is found within a few yards of the typical soda-granite.

Another kind (*Db*) is a fine-grained black rock, often small-porphyritic. It owes its color and texture to its having black mica in fine scales as its chief constituent. Like the preceding, it contains little quartz, and the feldspar is almost wholly

[10] This rock looks like the noryte, but contains augite in place of hypersthene. If its feldspar is chiefly labradorite (a point yet in doubt), it does not differ in mineral constitution from doleryte or diabase, or a prominent part of the so-called gabbro.

triclinic. Hornblende and augite are sparingly present. The rock is therefore a micaceous variety of the soda-granite. But though ordinarily massive, it sometimes has distinct indications of bedding. The rock is most common in the vicinity of limestone belts. It occurs at Centerville, east of limestone number 4 (see map), and also north of this limestone in the field west of the road, where it is conformable with the limestone.

E. *The Chrysolitic rocks.*—The chrysolitic rocks of the region have no resemblance in aspect to ordinary chrysolitic volcanic or igneous rocks. The kinds are (1) *chrysolitic hornblendyte*, (2) *chrysolitic pyroxenyte*, (3) *chrysolitic noryte;* and these graduate not only together but also into a rock in which chrysolite is the chief constituent. They are black or brownish-black rocks, and are mostly coarsely crystallized, the hornblende crystals being often an inch in length or breadth; and the chrysolite is in grains of various irregular forms and sizes, distributed through these crystals as well as among them, and not in well-formed crystals. In general, they contain but little feldspar, and this is triclinic; and a variety intermediate between the hornblendyte and pyroxenyte is common. They contain occasionally black mica, but no quartz. The chrysolite is more or less altered, as is shown (when examined in thin slices) by the bordering and intersecting bands of magnetite and viridite, and in some cases it appears to be changed to serpentine."

These rocks are largely exposed along the western half of the north side of Stony Point, west of the boat pier (the area is lettered *z z'* on the map), and over Montrose Point, as well as in its vicinity; at which places they are associated with noryte and other rocks of the series. They also outcrop in eastern and southern Cortland. The most eastern locality observed is within half a mile of the eastern border of the town, near the middle of the three "emery" mines referred to beyond, and the most southerly, a short distance east of Croton, within half a mile of Croton River.

The chrysolitic rocks are the most decomposable of the series, and wherever the brown-black ledges are crumbling in an extraordinary way and making a profusion of brown sand or brown or red earth, the presence of chrysolite may be suspected.

F. *Iron and Emery Mines.*—This Cortland region has its mines of magnetite, some of which are also mines of emery. The containing rock is either noryte, dioryte, or soda-granite,

[11] These chrysolitic rocks usually have, on a fresh fracture, the cleavage surfaces of the hornblende or augite spotted with chrysolite; but the presence of chrysolite, however abundant, cannot be made certain without slicing for microscopic examination, since the chrysolite is slightly altered externally, and such spots on hornblende crystals may be due to small imbedded crystals of augite. If the cleavage of the hornblende has an unbroken surface it is probable that the rocks contain no chrysolite. The hornblendyte has much stronger luster than the pyroxenyte.

and even chrysolitic rocks are sometimes near by. The iron ore has been found at several points within a mile north and northeast of Cruger's, and also three or four miles distant in the eastern part of the township of Cortland; but the amount appears to be small and no workings have yet proved profitable. The ore is commonly very chloritic, and contains less magnetite than the appearance and weight seem to indicate.

At a mine three-fourths of a mile north of Cruger's (at *u*, on the map, p. 195), the including rock is a dark reddish-brown noryte. The ore contains much chlorite, as shown by the gray-green tinge of its powder, and the green color of transmitted light when in very thin slices. With it there are also garnet and some fibrolite in minute short needles. Southeast of this locality (at *v* and *w* on the map) other openings have been made. Thin magnetite beds occur also on Cruger's Point, in the soda-granite and quartz-dioryte, half a mile west of the railroad station. The material is fine-grained, nearly black in color, chloritic like the preceding, and is usually associated with black mica. These beds are the subject of special descriptions beyond.

Among the localities in *eastern* Cortland, three are situated in a ridge or mountain running northward from Colabaugh Pond. One is at the southern end of the ridge, just north of the pond; another, near the road crossing it, about a mile farther north; and the third, at the north end of the ridge, nearly three miles from the pond, south of "Summer Hill." The magnetite, at each of these places, contains some disseminated corundum, making it a serviceable emery, and two of these mines have been worked for the emery; much of it also is chloritic. Fibrolite in small needles and divergent tufts is found with the ore at each locality.

B. The relation of these Cortland rocks to the other rocks of Westchester County.

The above brief description of the Cortland rocks prepares the way for a consideration of their relation to the other rocks of the county. The following questions arise: Are they one with the latter in system? are they rocks of an earlier system? or are they eruptive rocks, and not metamorphic, and, hence, of no bearing on the general question as to the age of the Westchester limestones and the associated schists? If it can be shown that the second or last supposition is the true one, the subject before us is rid by a stroke of the most serious of its perplexities.

1. Evidences of more or less complete fusion.

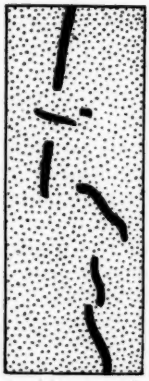
The evidences of fusion or plasticity are many; and, taking them collectively, they are decisive. They are exhibited in

the following ways: (1) The massive character of the crystalline rocks over so large an area, and a general resemblance in them to igneous rocks; (2) the great size of the hornblende crystals in some of the quartz-dioryte, and the well-defined crystals of hypersthene in part of the noryte, resembling the augite crystals of some volcanic rocks, facts indicating freedom of molecular movement during the process of crystallization; (3) the broken condition of the crystalline individuals in some places, which is evidence of movement while in a pasty state after the beginning of solidification; (4) the occurrence in the massive rocks of included fragments of other rocks, like the inclusions in many trap or basaltic ejections; (5) the existence of dikes or veins of the hornblendic and other rocks, of very various sizes, intersecting the adjoining rocks.

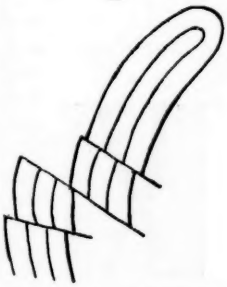
The inclusions are remarkably numerous in some portions of the region, and are often of wonderful magnitude. About Cruger's station, in the soda-granite and quartz-dioryte, they occur from an inch in breadth to many feet; one seen in the face of a bluff on the railroad, between three and four hundred yards northeast of Cruger's station, has a maximum breadth of eighteen feet and a length but little less, and consists of garnetiferous mica schist like that within a fourth of a mile to the east and south; and this is not the largest in that region. They abound also in the chrysolite rocks and noryte of Montrose Point and Stony Point, and in the limestone of Verplanck Point. They usually consist of the various materials which constitute the schist of the vicinity, even to the magnetitic garnet rock, quartzyte, etc.

Figure 3 represents ($\frac{1}{30}$ of the natural size) an example from the soda-granite, half a mile west of Cruger's, where displaced fragments of a thin layer of mica schist occur in the granite. Figure 4 ($\frac{1}{12}$ the natural size) is of an inclusion in the noryte of Montrose Point; the distorted form, the fractures, and the faults appear to be evidence of former free movement in the massive noryte. Figure 5 represents a surface three feet square from a large brecciated pyroxenyte adjoining directly the crystalline limestone on the shores of the Hudson at Verplanck Point. The masses in this strange breccia are contorted fragments of the limestone, one to two feet long, the thin layers of which have been brought out prominently by surface erosion.

3.

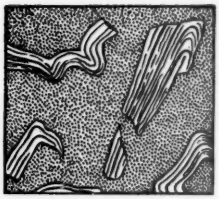


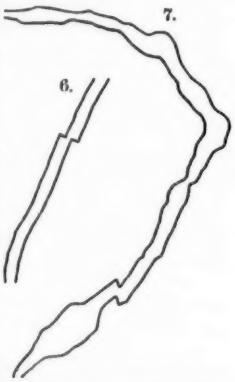
4.



The examples of what appear to be veins or dikes are also numerous. They cut through the chrysolite rock and noryte of Stony Point and Montrose Point, and through the crystalline limestone of Verplanck Point. Those at the last-mentioned place, facing the river (north of the foot of Broadway), vary from an inch in width to over fifty feet. Some are simply faulted bands, like figure 6. Others have more irregular courses, as in figure 7, representing eight feet from a vein at Verplanck Point. Figure 8 shows a crossing of two small veins from the same limestone region. Some, if not all, of such veins must, therefore, be true veins or dikes; and are evidence as to the former fused or plastic condition of the material and its injection into fissures. Veins formed in this way are not veins of infiltration or segregation, that is, they are not due to the filling of fissures by material supplied slowly in solution or vapor; for no difference in coarseness of texture or structure exists between the rock constituting them and that of the massive rock elsewhere; they are just such as have been made by simple injection.

5.





There are also peculiarities in the exterior of inclusions, and in the walls of veins or dikes, in some cases, which favor the idea of fusion. At Verplanck, the limestone of the wall is often discolored for two or three inches, and sometimes penetrated by the material of the vein, or contains minute crystals of hornblende; and in other cases, the limestone is impregnated with the hornblendic or augitic material in irregular lines or bands, so that surface erosion has left a complexity of small curving ridges. The crystallization of the limestone adjoining the vein is sometimes coarser than elsewhere; though, in general, no difference is apparent. On a small point, just north of the region of veins, part of the limestone is of the coarsest kind, the crystalline grains over a fourth of an inch broad, while the larger part is very fine in grain, like the most of the Verplanck limestone—a fact that indicates the local action of escaping heat.

8.

Still more positive evidence, if possible, of fusion are shown

at the junction of the schists of Cruger's Point with the soda-granite, where the schist itself bears evidence of partial fusion and exhibits other contact-phenomena.

The proof of the crystallization of the rocks from a more or less perfect state of fusion or plasticity is thus complete.

2. Evidences as to condition of fusion.

But, admitting fusion or a plastic condition, the question still remains:

Were these once-fused rocks fused approximately *in situ*, that is, where, or near where, they now lie; or were they erupted through fissures from great depths below? that is, using Dr. Hunt's terms, are they indigenous, or are they exotic?

a. *The results are partly the same whichever the condition of fusion.*—If they were fused where approximately they now lie, that fusion must have come from accessions of heat, and such accessions may have resulted from the movement and friction connected with an upturning of the rocks; and it may hence have been one of the results, in that region, of metamorphic action at an epoch of general metamorphism; and if so, at the very time that these rocks became fused or plastic through the process, other rocks of the region, owing to less extreme metamorphic action, or to less fusibility, may have been left with their bedding unobliterated; just as much granite in New England and other countries received its crystalline condition in the same process and at the same time with the associated schistose rocks, the gneisses, mica schists, etc.

All the facts as to fusion which have been presented are consistent with either mode of origin, even to the inclusions and the dikes or veins.

(1) The veins or dikes have the same essential characters whether made one way or the other. As has often happened in the case of granitic rocks, and even granular limestone, the fused or plastic material, under the pressure attending the subterranean movements, would have entered and filled all fissures that might have been opened to it, and so have made veins or dikes having the sizes of the fissures were they large or small, and possessing also a uniformity of grain like that of ordinary erupted rocks.[12]

(2) Again, whatever the process of ejection, fragments, large or small, of any rocks adjoining such fissures might have become included in the fused or plastic material.

[12] In the writer's Manual of Geology (1880), veins of this kind are called veins of plastic injection, an abbreviation of the full statement that they were made by the injection of material rendered plastic or fused during a process of metamorphism. They are better called dike-like veins.

(3) Moreover, the contact-phenomena in the case of veins so formed may be as decided and extensive as in that of any dikes or erupted masses.

(4) Further, the evidences of fluidal movement exhibited in the broken condition of many of the crystalline grains would be the same. Such a fragmenting of grains taking place after the stiffening of incipient solidification requires but a moderate amount of movement, and this is all that such circumstances would admit of. One foot would suffice; thousands would be impossible.

(5) Again, the resulting rocks need not, and generally do not, differ in kinds from erupted rocks of deeper source. In such fusions in the course of a process of metamorphism, the thickness of the rocks undergoing common movement may have a depth of 20,000 feet or more, and the fusion, therefore, would not be superficial. The view that many of the ordinary erupted rocks are nothing but fused sedimentary rocks need not be here discussed. The improbability of the view comes from the improbability of any movements in the earth's crust being sufficient to fuse its own rocks or the overlying sediments. But the epochs of metamorphism are the times not only of the profoundest movements of the crust, but also of the most thorough upturning of sedimentary beds, and if these are ever melted through the friction of upturning, or by its aid, then would be the occasion for it.

(6) Veins made at such an epoch by the injection into fissures of any rock so fused might have any extent, even that of the whole depth of the rocks metamorphosed; for the fissures may be thus deep. And the material filling them, since it might be that of the bottom rocks, might be wholly unlike that of the rock on either side of all the higher parts of the fissure.

But while there may be these resemblances between the effects of metamorphism and those of deep-seated eruption,

b. *The results of fusion of sedimentary beds under metamorphic action may have distinguishing peculiarities.—First:* The kinds of rocks so resulting are likely to vary greatly at comparatively short intervals, because sedimentary beds often vary thus. They should not have that uniformity for scores or hundreds of square miles which often characterizes ejections that have come up from regions beneath the supercrust.[13] Sediments, and therefore sedimentary deposits, are liable to frequent and sudden changes as to material, which igneous outflows cannot imitate. *Secondly*, the rocks are likely to have no

[13] The term *supercrust* is used for that part of the earth's crust which has been made by sedimentation, the *true crust* being restricted to the part beneath which is a result simply of cooling.

columnar (basaltic) structure; because the fractures to be filled in such cases are fractures in rocks which are participating in the movement and which, therefore, are heated rocks, and not cold.

Again, the phenomena of contact and the facts as to inclusions, structure and superpositions may have distinctive peculiarities.

c. *The condition of fusion or plasticity in the Cortland region.*—To answer the question before us we have, therefore, to consider more closely than has been done the phenomena of contact of the schistose with the massive rocks over the Cortland region, the peculiarities of some of the inclusions, the characteristics of some of the so-called veins or dikes, and the characters of the rocks as to their transitions, structure, and relative positions.

3. Special facts from the Cortland region.

a. *Contact-phenomena between the schistose and massive rocks; facts connected with the inclusions; stratigraphical relations to the limestones.*—The facts with reference to inclusions and all contact-phenomena bear directly, as will appear, upon the question as to any stratigraphical relation in the Cortland rocks to the limestones; and they are, therefore, here taken from the vicinity of particular limestone areas.

(1) *The vicinity of Cruger's limestone area.*—The small limestone area near Cruger's, (see map), lies mostly to the south and east of the station; only a small portion about forty feet in greatest width borders the river west of it, beyond the first brick-yard (*l*), the rest of the westward extension of the limestone being beneath the river. The schistose rocks directly and conformably adjoin it on the north, the average strike of both being N. 70° E. and the dip 75° to the northward. In the southeastern portion of the area there is a twist in the whole to the northwest. The limestone is finely crystalline granular, mostly white in color, and over the hills to the eastward contains crystals of white pyroxene.

The schist north of the limestone has a thickness of about a thousand feet. Toward the limestone, it is a silvery mica schist containing a little black mica and an abundance of very small garnets. A hundred yards or so to the north it is staurolitic, the staurolite occurring in grains of a clear chestnut-brown color and rarely in distinct crystals; and it also in some parts becomes quartzose and consequently thick-bedded. There are, besides, seams containing much magnetite; and at one place an intercalation of a black micaceous rock containing some feldspar which is about equally orthoclase and a soda-lime species. After another hundred to a hundred and fifty yards north-

ward, in the course of which it becomes increasingly staurolitic and garnetiferous, and passes in places into a true gneiss, it comes to its end against soda-granite and quartz-dioryte. Thus within a breadth of only 250 to 350 yards, there is here a passage from a stratum of crystalline limestone through conformable schists, to the massive rocks along which we have to look for contact-phenomena.

The facts here described are mostly from three south-to-north sections: *Section* 1, 300 to 400 yards west of the Station (*l* to *n*, on the map); *section* 2, about 700 yards (*p*); *section* 3, about 900 yards (*q* to *s*).

In section 1, *l* to *m* is the schist; at *m* is soda granite, which becomes hornblendic twenty-five feet above, toward the road; and then at *n*, on the north side of the road, the rock is of coarse quartz-dioryte. (The locality *n* is that of the first outcrop of rocks on the road going northwest from the railroad station.) The contact-phenomena in this section are as follows.

In the first place, the mica schist is even in bedding against the limestone; becomes more and more contorted to the northward, or away from it; and is full of flexures of a yard or so in span for the last fifty feet or more south of the junction with the granite.

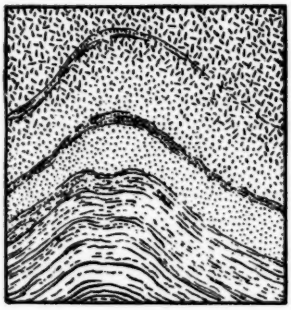
With the increase in the flexures of the layers, the schist becomes interlaminated with nodose-lines of quartz, vein-like in origin; and, besides, the garnets become somewhat larger. At the junction referred to, the schist is mostly a garnet rock containing much fibrolite and staurolite, and the latter is in some places granular-massive in a small way. Just below the granite, the layers are a compact body of flexures, and *in the soda granite* there is another flexed layer rather faintly indicated.

Figure 9 represents the condition here described; it was taken from the west side of a little bluff at *m;* the height is twenty feet. The dotted portion is that of the soda-granite. The garnet rock of the flexures under the granite contains, like the granite, soda-lime (or triclinic) feldspars, with little orthoclase; and the first foot of the granite is strongly garnetiferous;—facts which show a degree of transition in the material of the two rocks. The flexed bed within the soda-granite is gneissoid in character and of darker gray color than the granite; it is quartzose and garnetiferous, strongly micaceous with black mica, and contains magnetite and a little staurolite. The schist is consequently not a schistose portion of the granite, but a distinct bed; it is like the schist in its minerals, but in its more gneissic character indicates that it is intermediate between the schist and the soda-granite. The eastern face of the same ledge is about a dozen feet to the east of the western,

and here the junction of the schist with the granite looks more abrupt, but partly in consequence of erosion; above this plane

9.

10.



of junction, in the mass of the granite, distinct though fainter indications of flexed beds exist. The change above *m* from soda-granite to quartz-dioryte is simply a change in the substitution of hornblende for the larger part of the black mica, the feldspars being equally triclinic in the two, and the quartz equally deficient in amount. At a small bluff, 160 yards to the west of *m* (at *o*, see map), the change is more abrupt than between *m* and *n*; in only six feet, the rock passes from soda-granite to the dioryte.

A natural inference from the series of facts presented in this section, those as to the flexures in the schist as well as the changes at the junction of the schist and granite, would be that the heat of metamorphism increased from the limestone northward toward the granite and dioryte region, the heat being a consequence in part, if not chiefly, of the movement and friction attending the flexing, and that consequently there was produced a more and more yielding condition in the material of the schist as the region of complete fusion was approached, and, at the junction, perhaps a fusing and obliteration of portions of some layers of the schist; and that a bed of schist existed in the granite which approached somewhat the granite in character, but which, owing to the nature of its material, was not wholly obliterated.

But, are not these flexed portions of beds fragments that were broken off and carried up by the fused or plastic material as it rose from depths below? They lie so conformably to the flexures of the schist as to suggest a negative reply to this query.

Sections 2 and 3 (at *p*, and *q r s*, on the map) show inclusions on a grander scale.

Section 2 extends up the face of the first high bluff of bare rock west of *m* (a bluff that has by its east foot a path leading up among the trees to a fine spring).

Figure 10 represents a portion of the surface of the bluff about forty feet wide. Below is the hard contorted schist, a well-bedded micaceous schist, becoming in its upper part true gneiss; and above this, as the dotted surface shows, there is soda-granite, and then, after a few yards of this rock, the dioryte or hornblende rock, which is indicated in the diagram by short lines instead of dots. About a yard above the schist, *within the mass of the granite*, a schistose layer, about a foot thick, occurs; and eight to nine feet above this another *in the dioryte*, and *both are conformable to the schist.*

The upper bed of schist shows (in thin slices) that it is a quartzose, dark gray gneiss, containing much black mica and garnet, but also much triclinic feldspar and apatite, and in these two points approaching the soda-granite,—thus evincing a very marked transition in its composition toward that of the soda-granite. The first bed above it, lying in the granite, is similar to the schist in its black mica and quartz, but contains very little garnet; but like the soda-granite, it contains much apatite and more triclinic feldspar than orthoclase. Still other parallel beds are indicated at higher levels; one of them exists at the top of the bluff, twenty-five to thirty yards above the upper bed in the figure.

The facts look toward the same conclusions as those from section 1.

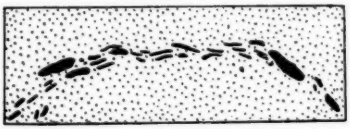
Section 3 was taken along a line about half a mile west of Cruger's Station, commencing on the river at *q* (see map, p. 195) in front of the most western of the brickyard sheds, and passing *r*, a point north of the upper shed, to *s*. For a distance of about 500 feet from the shore, the rock is mica schist; next follows soda-granite for about fifty feet; then, very coarse dioryte (the hornblende crystals in some parts finger-like in size) for 90 to 100 feet; then soda-granite again. At the shore the schist is nearly evenly fissile; 450 feet north, on the line of the section, it is like the six feet square represented in fig. 11. In the next fifty feet, the flexures are distinct but half faded out or nearly obliterated; and this is the last step before the soda-granite, the once plastic or fused rock, begins.

After twenty-five feet of soda-granite the first (*a*) of the ranges of "inclusions" appears; it is on the side of the road which here leads up the slope. Between three and four yards of the band are represented in figure 12. As shown, it is in pieces; yet the pieces are not much displaced, which they would be in an erupted rock. The material is grayish-black, and consists of a very chloritic magnetite, with a little black

11.

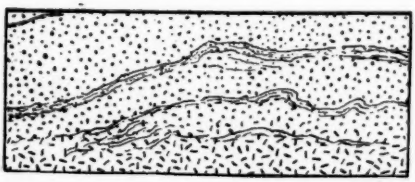
mica, in a feeble amount of base of triclinic feldspar. After an interruption it appears again for a short distance to the westward, where it is much more micaceous and garnetiferous; but the exposure is not as satisfactory as in the case of the other bands. Three feet behind this band, that is to the north, there is another similar one having a parallel position.

12.

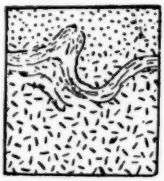


Eight or nine yards north commences the coarse dioryte. At the top of the slope, near the passage of the coarse dioryte to the soda-granite, partly in the dioryte but mostly in the granite, there are three bands (*b*) within three to five feet of one another, gneissic in constitution. Figure 13 represents about a dozen yards of these bands, in the dioryte and granite.

13.



14.



The upper or northern of the three bands (b^3) is exposed with small interruptions for *a length of more than one hundred and*

fifty feet; and the more eastern portion is shown in figure 14. The strike is the same as that of the schist.

The rock of the middle of these bands (b^3) is a quartzose gneiss, with black mica, many visible grains and octahedrons of magnetite, and some garnet—resembling the gneiss of some of the nearest schist and unlike the enclosing soda-granite in its excess of quartz, magnetite, and its garnets; and that of the others is similar. About three yards north, but a little to the west, is another band (b^4), one to two inches thick, which has a gray color, and consists of small spangles of silvery mica, some scales of black mica and chlorite, and grains of magnetite —a thin layer of the mica schist more magnetitic than usual.

About eight yards north of the third of the bands represented in figure 12, there is another schistose band (*c*) which is short in the line of the section, but appears again to the eastward and also to the westward; the rock is quartzose, garnetiferous, and includes chloritic magnetite with fibrolite, and other materials of the schist.

Eight to nine yards farther north, in the soda-granite, another band (*d*) exists, with the same strike—that of the schist—which outcrops for *two hundred feet*, or as far as the rocks in the direction are uncovered. This band is gray, like the last, and sparkles with the same pearly mica, but it is made up largely of brown staurolite in a half-granular form, showing but rarely crystalline faces, and contains also disseminated magnetite and some fibrolite and chlorite; a garnetiferous portion contains much black mica. Eight feet farther north, but to the eastward a few yards, a very silicious schist appears for a short distance. Fifty feet north, in the soda-granite, is still another band (*e*), which contains much chloritic magnetite; and a hundred beyond, another thin, gray, micaceous band resembling closely b^4; the condition of a portion of it is shown in figure 3, on page 203. Farther north, at intervals, other schistose bands exist, and along the north side of Cruger's Point, facing the brick-yards at the foot of Park street, they are more largely displayed.

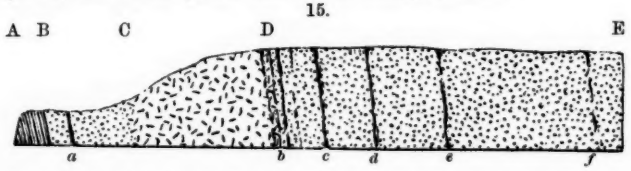
The extent of these bands, their number, uniformity of direction and apparently of dip, and the identity of the material constituting them with beds of the schist, especially the more northern, are such as to warrant the following section (fig. 15).

A B represents a portion of the schist near the granite; B to C, soda-granite; C to D, coarse syenyte-like dioryte; D to E, soda-granite; *a* to *f*, the bands, which are lettered as in the above descriptions of them.

The depth to which the beds are made to descend downward in the granite (100 feet) is an assumption in this section; but considering that probably 5,000 feet, and more likely

over 10,000 feet of these upturned rocks have been removed by erosion, and noting also the number of the bands and their parallelism to the schist, and the effect of pressure to keep them in place, the assumption can be no exaggeration.

15.



Since it is obviously impossible that the inclusions taken in and carried up by rocks erupted through deep fissures should be beds of schist 100 to 200 feet long, and a series of such beds separated by the fused rock retaining together their parallel position, we have to admit that these indications of bedding are of *unobliterated* bedding. The rest of the upturned strata were fused or at least softened; these portions of beds were not fused, though flexed and variously displaced.

There is reason for the resistance to fusion in the mineral nature of the beds; for quartz, staurolite, fibrolite, magnetite, are infusible minerals; muscovite and biotite are but slightly fusible on thin edges; and orthoclase fuses with great difficulty, much greater than the other feldspars, oligoclase, labradorite and albite.

Thus the study of the phenomena of contact becomes in this region a study of "inclusions;" and the larger of the inclusions turn out to be beds of schist, conformable to the schist.

We seem to be thus forced to the conclusion that the soda-granite and the included dioryte were once parts of the same sedimentary strata with the schist, and that all, with the Cruger limestone, were once a continuous stratified formation; and that the plasticity given to the granite-making or dioryte-making portions, because of the heat, occasioned the exceptional geological features of the region.

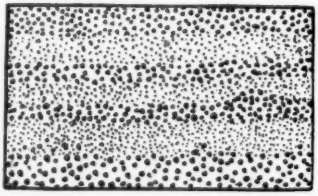
The region of Cruger's Point is continued northward into that of Montrose Point; the latter is characterized, as has been stated, by chrysolitic rocks for its southern three-fourths, and by noryte with chrysolitic rock for the other fourth; and through the facts there as well as elsewhere afforded, the evidence from inclusions is made to extend also to these other rocks. With the chrysolitic pyroxenyte and chrysolitic hornblendyte, there is also hornblendyte which is not chrysolitic, but more or less augitic and containing some triclinic feldspar. On the south side of Montrose Point facing Cruger's Point (or the brick-yard between the two), in the chrysolitic rock, there is what looks like a vein or dike two to four inches wide,

exposed for a length of twenty feet. The material shows it to be no vein or dike, but a bed from the schist; it is a dark-colored quartzose *garnet rock*, heavy with magnetite and containing some staurolite, resembling much a portion of the schist in section 1 (above described), near the soda-granite. The covering of earth prevented a determination of its whole extent. In the same kind of rock, about fifty yards north of the brickyard which divides Montrose Point (into a North Montrose and a South Montrose Point), a vein-like band, two feet to twenty inches wide descends the bluff, which consists of a light-gray massive argillyte. Examined in thin slices by the microscope, it is found to have a mealy aspect with microlitic points, like an argillyte in the first stages of metamorphism. The band is a bed from the schist, although a different variety of it from any exposed at Cruger's.

Other long vein-like bands are black and of very fine grain; some of them look grayish and minutely arenaceous. The microscope shows, on an examination of thin slices, that some consist of grains of hornblende and feldspar, the latter partly orthoclase, and look like hornblende schist, while others are very fine-grained hornblendic mica schist. One of the latter had a thickness of two feet. These bands are most numerous in the noryte of the northern part of the point. Figure 4, as stated on page 201, represents an "inclusion" in the noryte; but the inclusion is evidently a *bed bent back on itself;* for a vein would not be thus folded double in its enclosing rock. The rock of this *bed* much resembles the noryte, though finer in grain, and consists (as observed by means of a thin slice) of hornblende with much augite and some triclinic feldspar.

On the same part of the point, the noryte and chrysolitic rocks apparently cut through one another, but with the noryte oftener like an inclusion in the chrysolitic rocks. Again, they follow one another, or lie side by side, but without a distinct divisional plane: and in one place the rock consists of bands of noryte and chrysolitic hornblendyte without a trace of any planes of separation. Figure 16 represents an example of this kind, in which the bands are two to three inches wide, noryte bands (the fine-dotted in the figure), alternating with bands of the chrysolitic rock. Difference of material in successive portions might, under some metamorphic conditions, give rise to such a structure, although the bands are so thin; successive outflowings of different eruptive rocks could not produce it.

16.



Going north from the vicinity of Cruger's Station along section 1, instead of section 3, the rocks change from the coarse dioryte at the end of the section to fine-grained; and then, in three-fourths of a mile, the rock is well-characterized noryte. Moreover the noryte contains a band of magnetite, exposed in a working (near Mrs. Murden's) with which occur garnet and fibrolite. The band is bedded, the magnetite is chloritic, and the assemblage of minerals is the same that occurs in the soda-granite, as well as the schist west of Cruger's. Fibrolite is found also with the magnetite of Eastern Cortland.

The noryte and chrysolitic rocks are thus apparently in the same category with the soda-granite and quartz-dioryte.

Stony Point.—This conclusion is further sustained by the facts to be observed at Stony Point, and these facts come into this place although the locality is on the west side of the Hudson; for the Cruger schists make the south border of the region precisely as near Cruger's, and have the same strike and dip, showing a like relation to the Cruger limestone belt and proving its former extension across the river. For further comparison between the geological facts of the east and west sides of the river, it is to be observed that the succession of rocks west of Cruger's, on the line going northward, from the river on the south side of the point to the north side of Montrose Point, is (1) limestone; (2) schist; (3) soda-granite (with some included dioryte); (4) chrysolite rocks; (5) (on Northern Montrose Point) noryte and chrysolitic rocks in complicated combination.

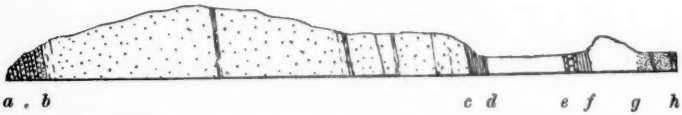
The same is the order on Stony Point, except that the limestone is not in sight (no doubt because submerged); it is: (2) schists; (3) soda-granite; (4) chrysolite rocks, followed by (5) noryte and chrysolite rocks combined. (The dioryte of the Cruger soda-granite is not represented there.) On the map, x is the area of the schists; y, the soda-granite; z, the chrysolitic rocks, and z' the latter with noryte. But besides being the same in order, there is evidence that the soda-granite succeeds the schist *along a plane of bedding* of the schists, as if conformable. This is apparent at the junction of the two on the east-northeast shore of the point. Included beds of schist occur, but the covering of earth prevents a determination of their direction. Further, the chrysolitic rocks succeed to the soda-granite along a plane parallel to the same plane of bedding, as is seen just west of the boat-pier near the middle of the northern shore. Besides these facts, there are included beds of fine-grained hornblende rock (schist?) and other kinds in the noryte and chrysolitic rocks, which are in general conformable to the same plane, or about N. 70° E. in strike, with a dip of 75° to 80° to the northward.

Such facts sustain the inference as to the former connection of the rocks of the east and west sides of the river, and strongly favor the view that the succession in the rocks noted was dependent originally on stratification.

If the thickness of the schists at Cruger's Point may be taken as that at Stony Point, the submerged Cruger limestone is to be found beneath the bottom mud of the river within a few hundred feet of the southeast shore.

(2) *Vicinity of the Peekskill Limestone areas.*—The Peekskill limestone areas have similar stratigraphical relations to the noryte and the other Cortland rocks. One of the two areas extends up Sprout Brook or Canopus Hollow, and the other up the valley in the village of Peekskill along which Center street descends toward the river. The southern extremities of these areas are shown on the map, page 195; the former has the strike N. 52° E. and dip 75° S.; the latter, N. 78° E. dip 75° S. Figure 17 represents a section about 1300 yards in length from

17.



north to south, along the line marked *a b c* on the map, starting from the limestone at the mouth of Sprout Brook, near the Iron works. The limestone (*a*) lies against true, whitish, well-bedded, conformable quartzyte (*a* to *b*); this quartzyte changes gradually to jointed massive granitoid and gneissoid quartzyte, with only an occasional bedded band or plane; each such band or plane is conformable in direction to the limestone and the adjoining bedded quartzyte. This quartzyte (the rock referred to on page 24 of this volume) continues southward to the Center street valley, but on the north side of the valley, just back of Hill's Foundry, it is followed by an arenaceous mica schist (*c d*), with the strike varied to N. 78° E. the dip remaining the same; then, on the south side of the valley, 50 yards above Baxter's Iron Works (on Water street), the limestone of the *second* belt outcrops, having the same dip and strike as the mica schist; and behind these iron works, thin-fissile dark-gray mica schist (containing both white and black mica) appears conformable in position to the limestone; then, after an earth-covered interval of about 200 yards, there is an outcrop of massive noryte along South street, north of Hudson street, which is without bedding, but has an extremely micaceous layer—a kind of coarse mica schist—intersecting it near its middle which is *conformable in its strike and dip with the mica schist and limestone of Center street valley;* and it shows conform-

able planes of bedding also near its south extremity. Moreover, this noryte has a lighter-gray color and contains more quartz and orthoclase than in other outcrops more remote from the mica schist and limestone, and thus exhibits an intermediate character corresponding with its intermediate position. This stratification in portions of the noryte is also distinct a mile to the eastward of this locality on the same side of the limestone, at the point mentioned on page 194.

The granitoid and gneissoid quartzyte of Peekskill *looks* much like true granite and gneiss; but its transition to bedded quartzyte shows what it in fact is; and this is confirmed by the examination of thin slices, the quartz in it proving to consist of an aggregation of grains just like a sandstone. (The transition of this granitoid quartzyte to schist or slate has been mentioned on page 24.)

These facts are all in favor of the conclusion that the noryte was once a stratum conformable to the Peekskill limestone areas.

(3) *Vicinity of the Verplanck Limestone belt.*—The Verplanck limestone belt follows the border of the river from a point just north of the foot of Broadway, and has the usual strike for the county, northeastward. Like the Cruger limestone area, it has, on the landward side, with a small exception, a border of ordinary mica schist or micaceous gneiss, a fine-grained arenaceous rock, the feldspar of which is mostly orthoclase. This schist extends to the point marked *d* on the map; at *c* the rock is massive noryte, but no junction of these two rocks is here in sight.

The exception referred to is at the southwest extremity of the belt on the river. Here there lies against the eastern side of the limestone a great mass of grayish or brownish-black rock of the Cortland series. It is mostly pyroxenyte, moderately coarse in grain, but varies to a kind in which the augite individuals are half an inch broad, and, on the other hand, to a fine-grained variety; and it contains, besides augite, a little hornblende, quartz, calcite, and apatite. But portions of the mass consist of coarsish hornblendyte; and a small part of micaceous augite-noryte; and there are also broad and narrow bands of very fine-grained black hornblendic mica schist, not showing well a schistose structure, part of which are conformable in strike and dip with the beds of the limestone, while others are in other positions. All the material is very pyrrhotitic. Besides, it contains the remarkable limestone breccia, of which a portion three feet square is represented on page 202.

This singularly constituted mass shows no appearance that looks like a subdivision into dikes or veins, except in the

bands of hornblendic mica schist: one of these bands has a border of the micaceous augite-noryte just mentioned. In the limestone just north of this mass, facing the river, occur the supposed dikes or veins mentioned on page 202. Some of them consist of pyroxenyte; others of coarsish hornblendyte; very fine-grained hornblendic rock looking like hornblende schist (*Ae*); very fine-grained hornblendic mica schist; augite-noryte.

As already admitted, there is here abundant evidence of a former plastic state in at least part of this augitic and hornblendic material. Still, there are strong reasons for questioning the idea of its deep-seated origin. (1.) The variety in the constitution of the mass bordering the limestone and in the supposed dikes or veins is very unlike what is ordinarily found in regions of igneous eruption. (2.) The supposed veins or dikes are for the most part conformable with the bedding of the limestone and partake in its flexures, just as if they had been originally beds alternating with the limestone depositions. (3.) The impregnation of the limestone along the junctions with pyroxenic or hornblendic material, sometimes minute crystals, looks as if it may have been in part at least a result of mixture attending original deposition.

Further (4.), there is the decisive fact that these intercalated masses are represented to the northward by bands ten feet and less to over thirty feet in thickness, of a black fine-grained mica schist, very pyritiferous. Going from the Point, the first outcrops of interstratified schist and limestone occur after an earthy interval of 300 yards, and here the mica schist is *hornblendic*, a feature it loses to the northward. These beds of mica schist have just the positions of most of the supposed "veins," and appear to be their more northern portions; and further, among the more northern "veins" of the Verplanck shore some are *simply mica schist*. Such facts explain also many of the vein-like bands of Montrose and Stony Points. The difference in mineral constitution between such interstratified beds to the northward and on the shore is what should be expected; for the limestone is bordered to the eastward in the one case by true mica schist, and, in the other, by augitic or hornblendic beds; and the associations at Montrose and Stony Points are similar. This view is also sustained by the occurrence in the limestone near the schist, 1000 yards from the Point, of coarse spots of pyroxene with mica and chlorite, rudely in layers, which must be due to the original deposition of impurity and metamorphic action. The augitic rock (pyroxenyte) on the east of the limestone at the Point outcrops (owing to excavations in the drift) for 200 yards from the shore; but its place beyond this continues covered for three-fourths of a mile, and here the rock is arenaceous mica schist; the spots of pyroxene are its only representative.

The essential continuity of these intercalated beds of mica schist with the intercalated beds of augitic and hornblendic material along the coast proves identity of origin, and origin by sedimentation. It indicates also a small change of constitution in the beds as they extend in that direction. The plasticity occasioned in part of the latter, during the progress of the metamorphism, accounts for all that looks like eruptive phenomena, even to the broken feldspar grains found in a slice of the pyroxenyte of one of the so-called veins. There is nowhere evidence of injection into or through cold rocks.

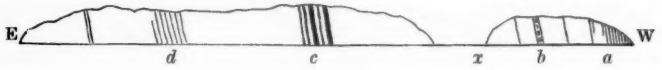
(4) *Vicinity of the smaller limestone areas of the Verplanck Peninsula.*—Six small limestone areas occur in the Verplanck peninsula. They are lettered on the map 1 to 5 and *j*. Number 3 has the strike of the large Verplanck belt, and has about it the same arenaceous mica schist. The others are in the midst of, or adjoin, the noryte, dioryte, and chrysolitic rocks, and hence might be put down among the "inclusions" of the region. In addition, they have a *northwest* strike (N. 17°–40° W.) But this is the strike, in part of Cruger's limestone area, and in a portion of the Verplanck belt, so that the twist is not confined to them. And, among so extensive masses of rock that became plastic or fused in the era of upturning, this abnormal position of included strata is not strange. Numbers 2 and 5 are probably parts of one belt; and numbers 1 and 4 may be in the same line, though disconnected by intervening rocks. The following are some of the stratigraphical facts observed among them.

No. 4, at Centerville, has at middle on its east side the compact porphyritic mica rock, *Cb*, which is schistose directly adjoining the limestone in the field west of the road, and has the strike of the limestone N. 47° W. But to the westward in the field (in which the limestone can be traced for 250 yards with a change of strike to N. 62° W.) the mica rock changes to hornblendyte and quartz-dioryte; and to the southeastward along the road, augite-noryte appears within a few feet of the limestone, both the feldspathic fine-grained (almost cryptocrystalline) variety (*Bc*), and the coarser dark variety. Whether this latter change in the bordering rock is due to a fault or not, could not be ascertained; it was not due to an intrusive dike.

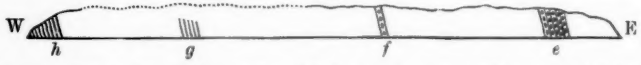
No. 5 outcrops on the railroad at 5 (see map) and on two roads at 5′ and 5″, with the strike N. 32° W. Between Montrose Station and this limestone at 5″, the rock is noryte, excepting some grayish augitic rock (*Bc*) at the corner (Munger's), where the road turns west, and an outcrop of *chrysolitic* noryte (hypersthene rock) 135 yards west of Munger's. Eighty yards beyond the chrysolitic rock comes the outcrop of limestone. One hundred and fifty yards west of the small exposure of

limestone, on the north side of the road, a ledge commences which extends along for nearly 350 feet, with no dike-like subdivisions. It *consists mainly of noryte and augite-noryte, but with some hornblendyte and noryte-gneiss, and has distinct planes of bedding in several places, all of which are conformable to one another.* The strike of its beds is N. 27° W., or nearly that of the limestone, and the dip 60° to 70° E. Part of the noryte is garnetiferous. Figures 18, 19, represent the stratification

18.



19.



observed in the ledge—fig. 18 the eastern portion, and 19 the western; forty feet of earthy interval separates the two. At the east end, at *a*, the rock (as a slice shows) is hornblendyte (with about equal proportions of orthoclase and triclinic feldspar), and it is schistose. It passes at the place to coarsish quartz-dioryte. At *b*, it is well-defined micaceous gneiss or noryte-gneiss, affording perfect observations of the strike and dip. West of this the rock is mainly noryte and augite-noryte. At *c*, for eight feet, black bands (or beds less feldspathic than the rest) alternate with the ordinary gray-black rock, and exemplify the conformability stated, though without divisional planes; at *d*, are divisional planes in the gray-black augite-noryte, having the strike N. 27° W. and dip 70° E. At *e*, is a much decomposed micaceous gneissic layer (noryte-gneiss apparently) conformable in strike, but varying in dip from 40° E. to 60° E.; at *f*, a distinct bed of light-colored very feldspathic augite-noryte, deeply decomposed, having the conformability; at *g*, or the west end, the rock is again micaceous and gneissic, with some garnets, the bedding distinct, and N. 27° W. in strike as at the east end.

This ledge, although made up mainly of massive noryte and augite-noryte, bears thus positive evidence of its having once had bedding throughout, and affords thereby a demonstration that its noryte is of metamorphic origin, and that the associated beds comprised also the limestone of the region.

(5) *A bed of quartzyte in noryte.*—About half a mile east of the limestone number 5, about the Montrose Station, the rock is the ordinary dark-colored noryte. 120 yards up the road going northeastward from the station, a bed of *whitish granitoid quartzyte* outcrops on the roadside for seventy yards, first on the west and then on the east side. This bed of quartzyte *overlies*

noryte. The noryte near the quartzyte is micaceous, quartzose and schistose, and that underneath is of the ordinary massive kind. There is evidence also that the bed of quartzyte has noryte above as well as below it. The quartzyte looks somewhat like a pale quartzose porphyritic granite; but, as observed in thin slices, the quartz consists of aggregated grains like sandstone; showing a resemblance to the Peekskill quartzyte. The feldspar is mainly orthoclase.

C. Conclusions as to the Cortland Rocks.

Many more observed facts might be here reported. But the above appear to be sufficient to settle the question as to the relations of the rocks of the Cortland series. They appear to sustain fully the following conclusions:

(1) These rocks, although they include soda-granite, noryte, augite-noryte, dioryte, hornblendyte, pyroxenyte, and chrysolitic kinds, are not independent igneous rocks erupted from great depths.

(2) However complete their former state of fusion or plasticity may have in some cases been, they are metamorphic in origin.

(3) The strata that underwent the metamorphism were one in series and conformability with the adjoining schists and limestone, and were part of the Westchester limestone series. (They are *younger* rocks if of different age, since they contain and intersect portions of the Verplanck limestone.)

(4) These Cortland rocks differ from the other Westchester County rocks because the metamorphic process had to do with sedimentary beds that differed in constitution or were in some respects under different conditions from those that existed elsewhere.

On the view reached, it follows that the limestones, schists, and other rocks of the Cortland region originally constituted together one series of horizontal strata. They underwent an upturning through subterranean movements, and in the course of it, they became metamorphosed; part into mica schist and gneiss, part, by loss of bedding, into the massive rocks. The number of these rocks does not imply widely different ingredients in the original strata. For hornblendyte and pyroxenyte have the same chemical constitution; the chrysolitic rocks contain no ingredient not in them also, and are peculiar mainly in their less proportion of silica. Moreover, the dioryte, noryte and augite-noryte are alike in containing the same bases in nearly the same proportions. The soda-granite differs in chemical constituents only through its mica, which indicates the presence of potash; but the other rocks also are often

micaceous and contain in addition more or less orthoclase. Silica, alumina, iron protoxide, magnesia, lime, soda, potash, are all the essential ingredients obtained in analyses of these various rocks (excluding the magnetite, apatite, pyrrhotite* and pyrite); and it is not mysterious, therefore, that such rocks should be among the results of metamorphism.

The title of this paper might, therefore, well have been

Soda-granite, noryte, dioryte, hornblendyte, pyroxenyte, and various chrysolitic rocks made through metamorphic agencies in one metamorphic process.

The geologist will nowhere on the continent find a more instructive spot for a day's walk than in the western portion of the Cortland region. Starting from Cruger's Station (37 m. from New York City), a walk of half a mile brings him to the western brick-yard shed; going north from here by a wood road carries him along section 3, and in less than a mile in a northerly direction (passing brick-yards at the end of it) he will reach Montrose Point and the chrysolitic rocks; following these around by the shore for about a mile he will then pass a brick-yard on the point, and beyond it find the norytes and chrysolite rocks together; a mile and a half more (passing on the route the large Verplanck ice-house) will take him to Broadway, in the village of Verplanck, near the foot of which street, on the shore, occur the limestone and its associated augitic and hornblendic rocks. Thus, in a distance of seven miles, he will see a wonderful diversity of rocks and facts. Possibly he may be convinced, at the end of the walk, that igneous eruption explains every thing. But let him go over this ground a second time more carefully, then trace the rocks of Verplanck Point northeastward, and afterward extend his walks in other directions over the Cortland region, and he may see enough to satisfy himself finally that, although there has been fusion and some eruption, it was not eruption from the earth's deeper recesses, like that which brought up trap (doleryte) through a series of great fissures for a thousand miles along the Eastern Atlantic border, from the Carolinas to Nova Scotia, all of it rock of one kind essentially, but eruption from less depths, not greater than the lower limits of a series of formations that were subjected together to foldings, fractures, and metamorphic change, and mostly far short of this.

The next subject is the distribution of the limestone areas of the county.

* Pyrrhotite is the common iron sulphide of the norytes, hornblendyte, pyroxenyte and chrysolitic rocks, and it is abundant also in the mica schist that occurs interstratified with the Verplanck limestone.

[To be continued.]

ART. XXVI.—*The Permian and other Paleozoic Groups of the Kanab Valley, Arizona;* by C. D. WALLCOTT.

[Taken from the report of fieldwork by permission of the Director of the United States Geological Survey.]

THE Kanab Valley heads on the high divide between the Colorado and Salt Lake Basin, and extends southward, seventy miles, through Southern Utah and Northern Arizona to the level of the river in the Grand Cañon of the Colorado. It is somewhat open in its upper portion before entering the terrace Cañons of the White and Vermilion Cliffs. Opening out below the latter into a low broad valley, it breaks through the escarpment of the Shinarump Cliff, and narrowing a few miles to the south, enters the Cañon worn down through the Carboniferous and Silurian formations to the depths of the Grand Cañon.

During the field season of 1879 a detailed section was taken of the strata exposed along its entire course. The section embraces 13,300 feet of bedded rocks ranging in time from the Lower Tertiary to the Upper Primordial.

The strata are conformable by dip, although unconformity by planes of erosion occurs in several instances.

In the present note the Permian and other Paleozoic groups will be spoken of as they were observed south of the Shinarump Cliff and in the lower Cañon of the Kanab Valley.

The accompanying tabulation of the subdivisions of the Paleozoic gives the general stratigraphical features of this portion of the section.

The Permian group terminates above with ripple-marked, banded, reddish-brown, and chocolate-colored arenaceous shales and sandstone. A plane of unconformity by erosion separates it from the overlying Shinarump conglomerate, which is considered as the base of the lowest Mesozoic group. It is undoubtedly of Triassic age, but, as yet, this has not been determined by paleontological evidence in the Colorado Valley.

The chocolate arenaceous shales give way below to drab or lavender-colored arenaceous and gypsiferous marls and shales, that pass, midway of the group, into reddish-brown shales of the same general character. A thin stratum of impure lime stone is intercalated in this bed forty-four feet above the summit of the lower division and fifteen feet above a band of impure shaly limestone. This band of limestone is of variable thickness and character, and forms the base of the upper division. Numerous fossils occur in it and the associated arenaceous layers.

The summit of the lower division was slightly eroded antecedent to the deposition of the limestone. The upper bed is a

	PLANE OF UNCONFORMITY BY EROSION.	
PERMIAN. 855 feet.	Upper Permian. 710 feet.	Gypsiferous and arenaceous shales and marls, with impure shaly limestones at the base.
	PLANE OF UNCONFORMITY BY EROSION.	
	L. P. 145 ft.	Same as upper division with more massive limestone at the base.
	PLANE OF UNCONFORMITY BY EROSION.	
CARBONIFEROUS. 3,260 feet.	Upper Aubry. 835 feet.	Massive cherty limestone with arenaceous gypsiferous bed passing down into calciferous sandrock.
	Lower Aubry. 1455 feet.	Friable reddish sandstones passing into more compact and massive beds below. A few filets of impure limestone are intercalated.
	Red Wall Limestone. 970 feet.	Arenaceous and cherty limestone 235 feet, with massive limestone beneath. Cherty layers, coincident with the bedding, in the lower portion.
	PLANE OF UNCONFORMITY BY EROSION.	
	Devonian. 100 feet.	Sandstones and impure limestones.
	PLANE OF UNCONFORMITY BY EROSION.	
SILURIAN. 785 feet.	? 235 feet.	Massive mottled limestone with 50 feet of sandstone at the base.
	Tonto Primordial. 550 feet.	Thin-bedded mottled limestone in massive layers. Green arenaceous and micaceous shales 100 feet, at the base.

Entire thickness of Paleozoic, 5000 feet.

reddish-brown gypsiferous marl that becomes more arenaceous below, finally passing into a sandstone that rests on the chocolate and cream-colored limestone beneath. This has suffered extensive denudation by erosion and is now found only in patches from ten to twenty feet in thickness over limited areas, the sandstone filling the spaces between. Recent erosion has cut channels down through each to the formation beneath, which is a member of the Upper Aubry group. A slight plane of erosion with an entire change in the character of the rock separates the two groups.

The Permo-carboniferous of Mr. G. K. Gilbert* is the same as my lower division of the Permian. It is placed as a subdivision of the group, now that the beds above are known to be of Permian age.

The stratigraphy of the section shows a group separable into two divisions, defined above and below by planes of unconformity by erosion and a decided change in the character of the beds from those of the subjacent and superjacent formations. There is no physical break in the beds above the Permian limestone of the upper division before the conglomerate is reached. This stratigraphical arrangement is sustained by the evidence of the fauna found in the limestones and associated arenaceous layers in the upper division.

The genera *Myalina*, *Schizodus*, *Nucula*, *Aviculopecten*, *Murchisonia*, *Naticopsis* and *Goniatites* are represented in the lower chocolate-colored limestone. The fauna is distinct in specific character from that of the Carboniferous groups beneath, and more intimately related to that of the fossiliferous beds of the Upper Permian division. Mr. G. K. Gilbert obtained from this same horizon *Pleurophorus*, *Schizodus* and *Bakevellia* a group of shells, as he states, suggesting the Permo-carboniferous of the Mississippi Valley.†

Twenty-three genera represented by thirty-four species comprise the fauna of the upper division. Of these the following have strong Paleozoic relations: *Scolithus* ——*?*, *Lingula mytiloides*, *Discina nitida*, *Orthis* ——*?*, *Rhynchonella Uta*, *Terebratula* ——*?*, *Nucula*, 2 species, *Aviculopecten*, 3 species, *Myalina*, 4 species, *Naticopsis*, 2 species, *Pleurotomaria* ——*?*, *Macrocheilus* ——*?*, *Cyrtoceras* ——*?*, *Goniatites* ——*?*, and *Nautilus* ——*?*.

The Permian character of the fauna is more marked by the presence of *Pleurophorus*, 3 species, *Schizodus* ——*?*, 3 species of *Bakevellia* including *B. parva*, *Pteria* ——*?*, *Mytilus* ——*?*, *Rissoa* ——*?*, and the still more typical Mesozoic genera *Pentacrinus* and *Pileolus* ——*?*.

The Pentacrinus plates were discovered by Mr. Edwin E.

* Wheeler Survey, West of the 100th Meridian, iii, p. 177, 1875. Also, see Arch. R. Marvin's Report in same, p. 213.

† Ibid. p. 177.

Howell, below the Shinarump conglomerate in Southwestern Utah. They belong to a species distinct from *P. asteriscus* of the Jurassic. Three species are now known to pass from the lower division to the limestone of the upper division.

The Permian character of the fauna, taken with the evidence afforded by the stratigraphy, clearly establishes the Permian as a well-defined and distinct group in the Colorado Valley. It occurs at the same horizon as the Permian determined by Mr. Clarence King in Northern Utah, Western Colorado, and Southern Wyoming, fully corroborating the views advanced by him of the age of the beds resting on the "Bellerophon bed" of the Upper Carboniferous.*

The Permian as found in the Kanab section undoubtedly extends to the west, east and southeast in Arizona and New Mexico.†

Mr. Jules Marcou referred the beds resting on the Carboniferous at the crossing of the Little Colorado to the Permian. He says:‡ "This formation, which is placed between the Carboniferous and the Trias, corresponds without doubt, to the magnesian limestone [Permian] of England." The proof of this was entirely stratigraphical, but Mr. Marvin's discovery of Permo-carboniferous fossils at the same horizon and locality, and on the same horizon of those obtained by Mr. Gilbert, which are known to have come from the lower division of the Permian, as given in the present note, tends to prove that Mr. Marcou was correct in his original reference of the beds he mentioned to the Permian and entitles him to the claim he asserts of adding a new member to the series of secondary rocks in North America, although, elsewhere, he includes a portion of the Upper Aubry group in his Permian.

The Carboniferous formation is divided into three great groups, Upper Aubry and Red Wall limestones with the intermediate Lower Aubry sandstones. The contained fauna is of the Coal-measure type, except near the base, where there is an assemblage of forms uniting a few coal-measure species with a much larger proportion of a Lower Carboniferous character.

The Carboniferous rests on the slightly eroded surface of the Devonian formation. The Devonian beds are very variable in character, and of little vertical range. At their greatest development, when increased by being deposited in a hollow of the limestone beneath, there is but 100 feet of purple and cream-colored limestone and sandstone passing into gray calciferous sandstone above. Over the knolls of Silurian limestone the

* Exploration of the 40th Parallel, i, pp. 245, 246, and atlas maps of the same.

† Messrs. Marcou, Newberry, Shumard, Gilbert, Marvin and Howell all give information of this horizon.

‡ Pacific R. R. Report III, pt. iv. Resumé, p. 170.

upper beds alone extend with a thickness of from10 to 30 feet. The purple sandstones deposited in the hollows of the Silurian limestone are characterized by the presence of Placoganoid fishes of a Devonian type. The Silurian limestone was extensively eroded antecedent to the deposition of the superjacent Devonian beds. Hollows 80 feet deep are seen that were worn in the evenly bedded strata. The upper 235 feet may belong to about the time of the Carboniferous group. The 450 feet of mottled limestone and 100 feet of arenaceous, micaceous shales is shown to be of Primordial age by the presence of *Lingulepis prima*, *Conocephalites* and *Bathyurus* in the upper portion, and *Hyolithes primordialis*, *Lingulepis*, *Crepicepalus*, and the species found above in the lower beds.

The missing Silurian groups may not have been deposited in this region, or, what is quite probable, their representatives were removed in the period of erosion that followed the close of the Silurian time, and has left traces of its action in the hollows and irregular surface over which the Devonian beds were spread.

ART. XXVII. — *Preliminary Account of a Speculative and Practical Search for a Trans-neptunian Planet;* by D. P. TODD, M.A., Assistant Nautical Almanac.

Introductory and Historical.

THE suggested probability, on scientific grounds, that there revolves about the sun a second planet exterior to the orbit of Uranus, is not new. So early as 1834, when the foremost astronomers of the day were by no means settled in their convictions that even the greater portion of the then rapidly increasing residuals in the longitude of Uranus was due to the perturbing action of a single exterior planet, Hansen is credited with expression of the opinion, in correspondence with the elder Bouvard, that a single planet would not account for the differences between theory and observation.* Dr. Gould, however, in his *Report on the History of the Discovery of Neptune*,† says, "I have the authority of that eminent astronomer himself [Hansen] for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief." Professor Peirce's criticism of the investigations of LeVerrier, to the effect that his predicted orbit of Neptune was so widely discordant from its

* Memoirs Royal Astronomical Society, vol. xvi, p. 388.

† Published by the Smithsonian Institution, 1850.

observed orbit as to indicate that his computations did not pertain to the actual disturbing planet, elicited from him the reply that the perturbations of Uranus due to a possible planet exterior to Neptune might readily cause an uncertainty of 5″ to 7″ in the fundamental data of his research.

In 1866, the Smithsonian Institution published the general tables of Neptune, by Professor Newcomb. In the investigation of its orbit the author proposed: "3. To inquire whether those motions [of Neptune] indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such a planet." He concludes (page 73) that it is "almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century."

In 1873, the Smithsonian Institution published the general tables of Uranus, by Professor Newcomb. His success in the treatment of the theory of Uranus was such that astronomers generally may be said to have been satisfied, from the smallness of the longitude-residuals, that there existed no evidence of perturbative action upon Uranus other than that actually taken into account in the construction of the tables. It is well known, however, that since the publication of these tables the error of longitude has been on the negative increase, and the latest observations place the planet increasingly more in advance of its theoretic position.

Sometime in the spring of 1874, the first preliminary outline of the very simple method which I have here employed in the treatment of planetary residuals with reference to exterior perturbation, suggested itself to me. For more than three years, very little opportunity offered for consideration of the problem of a trans-neptunian planet, and I gave it merely desultory attention. In August, 1877, however, I began to devote the larger portion of my leisure time to the theoretic side of the question. It was soon evident that no certain hold upon any possible cause of exterior perturbation could be obtained from the residuals of Newcomb's tables. And I may remark here that I have consequently chosen the term *speculative* rather than *theoretic* as applying more fitly to the investigation which preceded the actual telescopic search.

The Speculative Search.

While the magnificent researches of LeVerrier and Adams on the perturbations of Uranus are masterpieces of analytic skill, I felt that they should not be taken as models in the present investigation—for two reasons:

(1) The residuals of longitude which must form the basis of

the investigation are not sufficiently well marked to justify the execution of so laborious a research, especially if it be found that a simple, rational treatment, unencumbered with the refinements of analysis, may be fairly interpreted as indicating the position of an exterior perturbing body with merely a rough approximation.

(2) Even in the case of Uranus, and the theoretic search for Neptune, where the residuals of longitude were very strongly marked, many of the elements pertaining to the disturbing planet which Adams and LeVerrier sought to determine theoretically, turned out afterward, when their real values became known, to have been indicated with only meagre precision. Much less should we now expect these elements to be given with any certainty in the case of a planet exterior to Neptune.

I was also much impressed by a remark in Sir George Airy's *Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus*—"I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence."*

This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed telescopic search, to determine whether that search was worth undertaking; and, if so, at what point, approximately, it was best to begin. I should remark, also, that this portion of the work, as an investigation to these ends, was never undertaken with reference to publication.

I.—Let us now consider, seriatim, the errors of the elements of the perturbed planet—errors which the very hypothesis of a disturbing body introduces, and which must have entered into the tables of the inferior planet, as constructed independently of unknown exterior perturbation: we consider what the effect of these errors may be, and how far it may be eliminated or subtracted from the residuals of the actual theory of the planet. These residuals are, of course, first corrected for any known error of theory or tables, or erroneous masses of known perturbing planets.

(1) *The error of mean distance of the perturbed planet.*—Any error of radius vector enters very largely into the residuals of heliocentric longitude, if the observations are made at any considerable interval from the planet's opposition. If it is suspected that the error of radius vector will vitiate the residuals of longitude, we may avoid its effect by passing to residuals of geocentric longitude. Or, we may confine our research to the mean residuals of observations near the opposition-points, and symmetrically placed with reference thereto. The effect of erroneous radius vector is thereby eliminated.

* Memoirs Royal Astronomical Society, vol. xvi, p. 398.

(2) *The error of periodic time of the perturbed planet.*—If the residuals are examined graphically, the eye will readily detect whether any correction to the periodic time is advisable. If, in general, the mean line of the residuals is nearly a right line, and makes a given angle with the line of zero-residual, it may fairly be concluded that the residuals need a correction depending directly on the time, the magnitude of the coëfficient of which is indicated by the divergence of the two residual-lines.

I had considered the problem only thus far when it occurred to me to apply the method, only partially developed, to the determination of an approximate position of Neptune from the residuals of Bouvard's Tables of Uranus, published in 1821. Taking also the residuals from observations up to 1824, and not permitting myself a knowledge of the longitude of Neptune at any epoch, a very little labor gave me an approximate position of the disturbing planet from which, it now appears, Neptune might easily have been found some twenty years in advance of its actual discovery.

When my work had advanced to this stage, a mere chance threw in my way a copy of Sir John Herschel's *Outlines of Astronomy*, (which I had never before examined): I at once observed that my treatment of the residuals of Uranus with reference to a planet exterior to Neptune was quite similar to his "dynamical" exposition of the perturbations of Uranus arising from Neptune itself. And I was further gratified to find that he had given a very full and lucid statement of the effect upon the longitude-residuals caused by errors of the third and fourth elements of the perturbed planet—the error of eccentricity, and the error of longitude of perihelion. I therefore adopted, without hesitation, the continuance of the graphical method therein detailed; and shall do no more here than to refer to the pages of Herschel's treatise where these elements are dealt with.

(3) *The error of eccentricity of the perturbed planet.*—(See Sir John Herschel's *Outlines of Astronomy*, page 536.

(4) *The error of longitude of perihelion of the perturbed planet.*—(Ibid., page 537.)

When the longitude-residuals have been corrected in this manner, we proceed on the assumption that any outstanding residuals are due to unexplained exterior perturbation.

II.—Of the seven elements of the disturbing planet, we must assume a value of one: the values of three others, together with the mass of the disturbing planet, we may consider as theoretically determinable from the longitude-residuals themselves.

(1) *The mean distance of the disturbing planet.*—Regarding

the next order of distance beyond Neptune as occupied by the planet for which we are searching, I assumed, as a first value of mean distance, $a=46{\cdot}0$: this value seemed to be indicated by a fair induction. The periodic time of the planet would then be 312 years, and conjunctions with Uranus would occur nearly at intervals of 115 years.

(2) *The eccentricity of the disturbing planet.*—Even with the large residuals of Uranus employed in the investigations of LeVerrier and Adams, the derived value of the eccentricity of Neptune was entirely illusory. The several values of eccentricity of Neptune resulting from their investigations are as follow:

Adams (*first hypothesis*)	0·16103
LeVerrier	0·10761
Adams (*second hypothesis*)	0·120615

The eccentricity given by investigation of the orbit of Neptune from observations of the planet was:

Newcomb (*Tables of Neptune*)	0·0089903

We should, therefore, expect nothing of any attempt to arrive at the eccentricity of an orbit exterior to that of Neptune.

(3) *The longitude of perihelion of the disturbing planet.*—Much the same remark obtains in reference to this element. The several values of longitude of perihelion of Neptune, resulting from the researches on perturbations of Uranus, are as follow:

Adams (*first hypothesis*)	315° 57′
LeVerrier	284° 45′
Adams (*second hypothesis*)	299° 11′

The longitude of perihelion given by observations of the planet is:

Newcomb (*Tables of Neptune*)	46° 6′ 39″·7

Evidently it would not be wise to include this element in the investigation.

(4) *The epoch of the disturbing planet.*—If we can obtain even a rough approximation to the value of this element, the end of the investigation is fully attained. An inspection of the outstanding residuals, graphically exhibited, will show. without further labor, the epochs of maximum disturbance. The best that can be done will be to prepare an approximate perturbative curve, the epochs of maximum disturbance of which shall be in harmony with the assumption of mean distance of the exterior planet. By applying this to the plot of outstanding residuals, we may decide at what points the appli-

cation of the perturbative curve best accounts for them. The amount of excursion in the several sinuses of the perturbative curve we need not for this purpose attend to with any great care: this will depend upon the mass and distance of the disturbing planet; and, that it will be unavailing to attempt any determination of the mass in the present case will be evident from the fact that the mass of Neptune, from the theoretical investigations of LeVerrier and Adams, was widely discrepant:

Adams (*first hypothesis*)	0·0001656	$\frac{1}{6039}$
LeVerrier	0·0001075	$\frac{1}{9300}$
Adams (*second hypothesis*)	0·00015003	$\frac{1}{6666}$

While the most reliable mass of Neptune from observation is:

Newcomb (*motion of the satellite*)	0.00005160	$\frac{1}{19380}$

We have thus reduced the inverse problem of perturbation to a very simple, rational form. The residuals of longitude of Uranus were next treated in accordance with this method.

In his *Investigation of the Orbit of Uranus*, Newcomb presents three series of residuals: the mass of Neptune finally adopted in the tables, $\frac{1}{19700}$, corresponds very nearly to the mean of the first and third series. But the mass of Neptune which was employed in this investigation is that given by Newcomb's discussion of the motion of the satellite of Neptune, and is $\frac{1}{19380}$. Our first step, then, was to correct these mean residuals into accordance with this adopted mass. The following table presents the date, the mean residuals, the correction for mass, and the corrected mean residuals.

Date.	$\frac{1}{2}(\Delta_1 l + \Delta_2 l)$.	Mass-correction.	Σ.
1691.0	−11″	+3″	−8″
1715.2	− 8.5	+1.9	−6.6
1751.1	+ 2.8	−0.7	+2.1
1769.0	− 1.5	+1.5	0.0
1783.3	− 0.17	+2.58	+2.41
1790.0	+ 0.76	+2.27	+3.03
1795.0	− 0.36	+1.73	+1.37
1802.0	− 1.06	+0·72	−0.34
1806.5	− 0.86	+0.09	−0.77
1810.5	− 0.21	−0.32	−0.53
1814.5	− 0.32	−0.64	−0.96
1819.5	− 0.37	−0.79	−1.16
1824.8	+ 1.45	−0.79	+0.66
1829.7	+ 0.86	−0.76	+0.10
1835.2	− 0.37	−0.77	−1.14
1839.8	− 0.33	−0.82	−1.15
1844.8	− 0.02	−0.93	−0.95
1849.9	− 0.37	−1.08	−1.45
1854.9	− 0.25	−1.20	−1.45
1860.0	− 0.14	−1.27	−1.41
1865.0	+ 0.49	−1.24	−0.75
1870.0	+ 0.12	−1.05	−0.93

The numbers, then, in the column Σ, taken in connection with the corrected residuals given by observations up to 1877, formed the basis of the subsequent investigation. I need only state here that, examining these residuals according to the method just related, in reference to unexplained perturbing action, I concluded that Uranus was in conjunction with an exterior perturbing body between the years 1780 and 1795, and that another conjunction would take place at some time before the close of the present century. The most probable position of the exterior planet I therefore considered to be about 170° of longitude: the probable error of the position I estimated to be, roughly, 10°. This result was reached on the morning of the 10th of October, 1877. During the few days immediately following, I reviewed this examination, as much as possible independently of the previous result, and at the same time varying the assumed mean distance. With a value of $a = 52.0$ (which I finally considered inductively the most probable) I set down the longitude of the exterior planet equal to $162° \pm 6°$: this result was reached on the afternoon of the 14th of October. I now turned my attention toward a similar treatment of the residuals of Neptune, with a slight hope of getting a confirmatory result: two suppositions agreed in fixing the longitude at about 180° and 200°, respectively. I therefore, on some day in the latter part of October, 1877, wrote down as the exposition of all my enquiry, the following results:

EXTERIOR PLANET.—Longitude (1877·84), $170° \pm 10°$
Mean distance from the sun, 52·0
Period of revolution about the sun, 375 yrs.
Mean daily motion, 9″·46
Angular diameter, 2″·1
Stellar magnitude, 13+
Longitude of ascending node, 103°
Inclination of orbit to ecliptic, 1° 24′

To the determination of the four latter results I shall allude presently. I may now add that this result for longitude of the exterior planet is sustained by observations of Uranus up to and including the late opposition. If a new, disturbing planet really exists in the longitude here indicated, nearly a century must elapse before its existence can be asserted at all positively from residuals of Neptune alone.

The Practical Search.

I should never have been able to execute the telescopic search consequent upon the investigation just related, had it not been for the courteous offices of Rear Admiral Rodgers,

Superintendent of the Naval Observatory, and Professor Hall, in charge of the great refractor. It was with this instrument—the 26-inch equatorial—that the search was conducted, beginning on the night of the 3d of November, 1877. It seemed to me that I should begin the search at a point about 20° preceding that indicated as the most probable position of the planet, and continue it to a point following by the same distance. But a careful search extending over a zone of this length, and of sufficient width to be certain to contain the supposable planet would be a work of such magnitude that I could not expect its completion under several years. I therefore had recourse to an inductive determination of the inclination and longitude of node of the planet's orbit.

I computed anew the position of the invariable plane of the solar system. A differential comparison of its inclination with the inclinations of the orbits of the major planets gave, with little uncertainty so far as the mere induction was concerned, the inclination of the orbit of the trans-neptunian planet equal to 1° 24′. Similarly I obtained for the longitude of node, though not so certainly, 103°. For the preliminary search, I determined to fix the latitude-limits of the zone at a width of one degree to the north, and one degree to the south of this adopted plane. To these elements I strictly adhered,—with the intention, however, of alternately increasing and decreasing the inclination, and varying the longitude of node, if I should arrive at no successful result from the search of this limited zone.

I may remark that the detailed plan of the instrumental search had been completely digested and written out as early as the 5th of September. To assist in a decision as to what method of search I should employ, I had recourse to an inductive consideration of the real diameters of the known planets of the solar system: I need only mention here that I arrived at the result that a diameter of 50,000 miles might be taken as the minimum value for a planet next beyond Neptune. On this assumption, the mean distance of 52·0 gave for its apparent diameter 2″·1. I did not, therefore, hesitate in adopting the method of search depending upon the detection of the planet by contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. I considered a magnifying power of 350 diameters sufficient for the detection of a disk of this diameter: in the actual search, a power of 600 was often employed, but most of the search was conducted with a power of 400 diameters.

On thirty clear, moonless nights, between the 3d of November, 1877, and the 5th of March, 1878, this search was carried on after the manner I have indicated.

After the first few nights, I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing-book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy all suspected objects. On the succeeding night of observation these objects were re-observed: and, at an interval of several weeks thereafter, the observation was again verified. At 3 A. M., the 6th of March, 1878, the search was discontinued—my observing-book ends with the following note:

"The adopted plane of orbit of trans-neptunian planet is now searched (without break) from

$$v = 146°{\cdot}8$$
$$\text{to } v = 186°{\cdot}1."$$

I have much confidence in this telescopic search—my aim was to sweep the zone so carefully that there should be no pressing need of duplicating it. If a trans-neptunian planet of an apparent diameter so great as 2″ is ever discovered, I shall be much surprised if it is found that it must have eluded my search.

Very soon after the termination of this search, I received the new tables of Uranus, by the late LeVerrier.* I at once instituted a treatment of the residuals of these tables after the method employed with those of Newcomb. I merely mention here that I reached a result entirely confirmatory to that previously obtained. The residuals were first reduced to Newcomb's mass of Neptune.

I ought not to conclude this paper without adverting to the apparently long delay of its publication. From the very beginning, I had approached the entire problem of search for a trans-neptunian planet with resolute direction toward the end which I regarded of the highest scientific import—that of *finding the possible planet at the earliest moment:* if I were successful, observations of its position would then be secured at once, and an accurate determination of its elements would be a matter of earlier realization—it seeming improbable that any prior chance-observation would ever be brought to light. After pursuing the theoretic side of the question for a short time, I saw clearly that many years must elapse before the perturbing action of this body on any interior planet would afford anything like pronounced evidence of its existence: recourse must be had to the practical, telescopic search. So I tarried longer with the residuals of Uranus only in the hope of a possible shortening of the search by some indication that the

* Annales de l'Observatoire de Paris, Mémoires, vol. xiv.

planet was more probably in one portion of the heavens than in another. After the telescopic search which I was conducting had been temporarily brought to an end, by circumstances beyond my control, I was not without hope of effecting some arrangement whereby I might resume the search at an early day, and carry it to a satisfying conclusion. After much thought upon the apathetic reception with which the magnificent researches of Adams and LeVerrier had met, I reached the conclusion that no competent observer would be led to continue the search through knowledge of the little work of speculation that I had done. And, as the work was undertaken with the end always in view of finding the planet, I saw no good to come from its publication.

It will be remarked that this matter now assumes a very different aspect: the publication of a recent memoir *On Comets and Ultra-Neptunian Planets*, by Professor George Forbes, of Glasgow, assigns, by a method of investigation entirely independent of my own, a position to a possible trans-neptunian planet which may be regarded as in exact coincidence with that which I have deduced. The assumption of a mean distance 100, indicated in Professor Forbes' paper, will not appreciably destroy the representation of the residuals with which I have dealt. I have not yet been able to convince myself that the remarkable harmony of the results of the two investigations is simply a chance agreement; and, with the hope that the accumulated evidence of the existence of a far exterior planet may not fail to incite some observer in possession of sufficiently powerful telescopic means to a vigorous prosecution of the search, I have prepared this preliminary paper in order that attention may be called to the matter in sufficient advance of the now approaching opposition-time. I may add here that, should a careful and protracted search of the region adjacent to the indicated longitude prove unavailing, no more certain test of the existence of a trans-neptunian planet admits of application within the next few years than that of telescopic search of a limited zone extending entirely around the heavens—a search which I have been hoping, for more than two years past, for an opportunity to undertake, but which I see no present prospect of realizing.

Nautical Almanac Office, Washington, August 4, 1880.

ART. XXVIII.—*Notice of Jurassic Mammals representing two New Orders;* by O. C. MARSH.

IN addition to the Mammals from the Jurassic deposits of the Rocky Mountains, already described by the writer,* a number of other specimens have since been discovered. Some of these add materially to our knowledge of those previously found, and others are different from any now known.

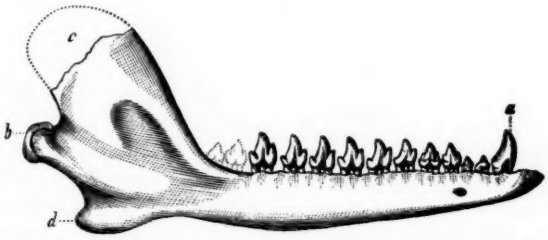
This new material is all from the Atlantosaurus beds, in essentially the same horizon which furnished the earlier specimens. The general resemblance of the American forms to those from the Purbeck of England becomes still more evident in the remains here described.

Diplocynodon victor, gen. et sp. nov.

One of the largest mammals yet discovered in the Jurassic beds of this country or of Europe is represented by various remains of several individuals, found in the same locality. The most characteristic of these specimens is a right lower jaw, with most of the teeth in position, and well preserved.

The general characters of this jaw are well shown in the figure below.

1.



Right lower jaw of *Diplocynodon victor*, Marsh. Outside view; twice natural size. *a*, incisor; *b*, condyle; *c*, coronoid process: *d*, angle.

This jaw is quite distinct from anything hitherto described, and exhibits several characters of special interest. There were at least three incisors, directed well forward. The canine is very large, and is inserted by two fangs. This important fact has suggested the name of the genus. The molar series consists of no less than twelve teeth, all essentially the same in form, and each inserted by two fangs. There are apparently six premolars. The second of these is smaller than the first, and the others gradually increase in size. The last true molar was smaller than the others. The crowns of these teeth are

* This Journal, vol. xv, p. 459, 1878; vol. xviii, pp. 60, 215 and 396. 1879.

composed of a main external cone, with a small elevated lobe in front, and a lower one behind. This is repeated on a reduced scale on the inner side, except that the posterior small cusp is rudimentary or wanting. The antero-posterior faces of the crown are deeply excavated, and grooved. There is no cingulum.

The jaw is elongate, and gently curved below. The coronoid process is large and elevated. The condyle is placed very low, nearly on a line with the teeth. The angle of the jaw is produced into a distinct process, the lower margin of which bends outward, although the process as a whole has a slight inward direction, which may be due to pressure.

A second specimen, apparently of the present species, is a left upper jaw, with the canine and eight succeeding teeth in excellent preservation. The canine is very large, and has two distinct fangs. The molar teeth have one main external cone, and two lateral cusps, which rise from a strong basal ridge. On the inner side, there is one main cone, with a small posterior heel. The outer face and the sides of the upper molars are deeply sculptured with irregular grooves.

Stylacodon validus, sp. nov.

Since the discovery of the type of this genus,* two other allied specimens have been brought to light, one of which, also a lower jaw, proves to be new. This indicates a species much larger than the one first described, but apparently belonging to the same genus. The molar teeth in this jaw are inserted by a single fang. The anterior premolars preserved have each two fangs. All the molar series in place have elevated conical crowns, raised some distance above the outer margin of the jaw. They have one main external cone and three inner cusps, thus agreeing in general form with the molars of *Dryolestes*. The mylohyoid groove is distinct, and continues forward nearly to the symphysis.

Some of the dimensions of this specimen are as follows:

Space occupied by eight anterior molar teeth,	10·0^{mm}
Depth of jaw below first lower premolar,	3·0
Depth of jaw below fourth premolar,	3·5
Height of crown of fourth premolar,	2·0

Tinodon ferox, sp. nov.

An interesting specimen of the genus *Tinodon*, about twice as large as the type, is a right lower jaw, with most of the teeth in position. There are three premolars of the usual form, and apparently four molars. The premolars have one main cone, pointed and compressed, with a low faint cusp in front, and a high larger one behind. The last premolar is very large. The

* This Journal, xviii, p. 60, July, 1879.

penultimate molar has four distinct cones instead of three. The canine was large, and directed well forward. The coronoid process is large, and inclined backward. The mylohyoid groove is nearly parallel with the lower margin of the jaw, and extends forward to the symphysis. The latter is large, and elongate.

The main measurements of this specimen are as follows:

Extent of lower molar series,	14·5^{mm}
Space occupied by premolars,	7.0
Depth of jaw below canine,	3.0
Depth of jaw below last premolar,	3·5
Depth of jaw below last molar,	4·0
Antero-posterior diameter of penultimate molar,	3·0
Height of crown,	2·5

Triconodon bisulcus, sp. nov.

Two or three fossil jaws in the Yale Museum agree so closely with the *Triconodon*, of Owen, from the Purbeck of England, that they may be provisionally placed in that genus. One of these specimens is a right lower jaw, with several teeth in good preservation. The molar teeth differ from those of the typical *Triconodon* in having the middle cone larger than the other two. The penultimate molar is the largest of the series. The angle of the jaw is not inflected, but the outer margin of the angle is developed into a rounded ridge. On the inner side, there is a strong sharp ridge running from the inlet of the dental canal backward to the condyle. The mylohyoid groove is well marked, and there is a second deep downward-curved groove beneath it.

The size of this specimen is indicated by the following measurements:

Space occupied by three last molars,	8·0^{mm}
Antero-posterior extent of last molar,	2·0
Depth of jaw below last molar,	4·0
Antero-posterior extent of penultimate molar,	2·5
Height of crown,	2·0
Depth of jaw below last premolar,	3·0

Dryolestes obtusus, sp. nov.

Additional specimens of *Dryolestes* show that this genus possessed a peculiar dentition. There were no less than twelve teeth in the lower jaw behind the canine, and at least eleven in same series above. The upper molars had three external cones and one inner cusp, and this order was reversed in the lower molars. There was no cingulum above or below. The canines were small.

The specimen on which the present species is based is a left upper jaw, with the molar series nearly complete. The crowns

of the true molars differ from those in typical specimens of *Dryolestes* in having the cusps blunted, making the crowns unusually short. Another important difference is that the tooth which may be regarded as the last premolar is so much larger than the rest of the series that it projects far beyond them. The line of the true molars is much curved outward.

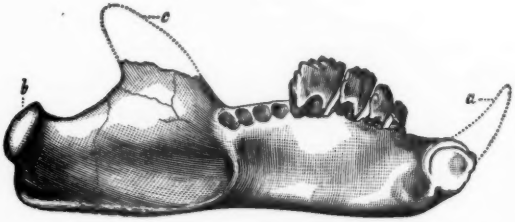
Some of the dimensions of this upper jaw are as follows:

Space occupied by eleven posterior teeth,	12·0^{mm}
Extent of seven posterior teeth,	8·0
Projection of last upper premolar from jaw,	2·0

Ctenacodon serratus, Marsh.

Additional remains of this interesting form have made clear some points in the structure of the lower jaw which the type specimen did not show.* The left lower jaw, represented in figure 2, agrees so closely with that specimen in size and general features, that it must be referred to the same species. The symphysial portion is very short, and nearly round. The inlet of the dental canal is quite large, and enters the jaw beneath a fold that curves downward, and passes into the inflected lower margin of the angle. The latter is distinct from the condyle, not identical with it as in *Plagiaulax*. The coronoid process is of moderate size, and directed well backward.

2



Left lower jaw of *Ctenacodon serratus*, Marsh. Inner view, four times natural size.

Mesozoic Mammals have been very generally referred hitherto to the *Marsupialia*. An examination of all the known remains of American Mesozoic Mammalia, now representing upwards of sixty distinct individuals, has convinced the writer that they cannot be satisfactorily placed in any of the present orders. This appears to be equally true of the European forms which the writer has had the opportunity of examining. With a few possible exceptions, the Mesozoic mammals best preserved are manifestly low generalized forms, without any distinctive Marsupial characters. Not a few of them show

* This Journal, vol. xviii, p. 396, Nov., 1879.

features that point more directly to Insectivores, and present evidence, based on specimens alone, would transfer them to the latter group, if they are to be retained in any modern order. This, however, has not yet been systematically attempted, and the known facts are against it.

In view of this uncertainty, it seems more in accordance with the present state of science, to recognize the importance of the generalized characters of these early mammals as at least of ordinal value, rather than attempt to measure them by specialized features of modern types, with which they have little real affinity. With the exception of a very few aberrant forms, the known Mesozoic mammals may be placed in a single order, which may appropriately be named *Pantotheria*. Some of the more important characters of this group would be as follows:

(1.) Cerebral hemispheres smooth.
(2.) Teeth exceeding, or equalling, the normal number, 44.
(3.) Premolars and molars imperfectly differentiated.
(4.) Canine teeth with bifid or grooved fangs.
(5.) Rami of lower jaw unankylosed at symphysis.
(6.) Mylohyoid groove distinct on inside of lower jaws.
(7.) Angle of lower jaw without distinct inflection.
(8.) Condyle of lower jaw near or below horizon of teeth.
(9.) Condyle vertical or round, not transverse.

The generalized members of this order were doubtless the forms from which the modern specialized Insectivores and Marsupials, at least, were derived.

Another order of Mesozoic mammals is evidently represented by *Plagiaulax*, the allied genus *Ctenacodon*, and possibly one or two other genera. These are all highly specialized aberrant forms, which apparently have left no descendants. This order, which may be termed the *Allotheria*, can be distinguished from the previous group by the following characters:

(1) Teeth much below the normal number.
(2) Canine teeth wanting.
(3) Premolar and molar teeth specialized.
(4) Angle of lower jaw distinctly inflected.
(5) Mylohyoid groove wanting.

These characters alone do not indeed separate the *Plagiaulacidæ* from some of the Marsupials, and future discoveries may prove them to belong in that group, where they would then represent a well marked sub-order.

Yale College, New Haven, August 7th, 1880.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Terrestrial Magnetism.*—Professor BALFOUR STEWART, in a letter to Nature, July 1, 1880, discusses the connection between auroras and magnetic storms. Since we have changes produced in stationary strata by a moving magnet, cannot the reverse be true? May we not have discharges produced in moving strata by a stationary magnet? The sun in this case would by convection currents produce changes in the atmospheric strata and the earth as a permanent magnet would cause electric disturbances, which in turn would react upon terrestrial magnetism. Working on this hypothesis Balfour Stewart has been led to the fact "that certain magnetic diurnal changes lag behind corresponding solar changes, just as meteorological changes would do," and he also states that his observations up to the present appear to show that an increase or decrease of solar activity corresponds to an increase or decrease of both magnetic and meteorological activity. The probability of a progress of magnetic phenomena from west to east, corresponding in character to a progress of meteorological phenomena is alluded to. Magnetic weather appears to travel faster, however, than meteorological weather. J. T.

2. *On the Reversal of Photographic Impressions.*—M. J. JANSSEN has discovered that photographic images can be made to pass from negative to positive by the prolonged action of the same light which produced them originally. At Meudon solar photographs are obtained with an exposure of about $\frac{1}{1000}$ of a second. These photographs show the granulations of the photosphere. With gelatine bromide of silver plates the time of exposure is in the neighborhood of $\frac{1}{20000}$ of a second. If, however, one of these dry plates receives an impression of light during a half a second or one second—that is to say, during a time ten to twenty thousand times longer than that which gave the first impression, the developer reveals a positive image giving the disc of the sun in white and the spots in black. This positive image can be obtained as distinct as the negative image which it replaces. A certain intermediate length of exposure gives an image intermediate between a negative and positive image, and the plate presents a sensibly uniform tint. If one allows the light to act for some time after the positive image is produced, this last image disappears in its turn and the developer causes no metallic deposit to be formed upon the image, which appears uniformly transparent upon the black background of the sky. This dark background disappears under the prolonged action of the light. A complete discussion of the results obtained is promised.—*Comptes Rendus*, No. 25, p. 1447, June 21, 1880. J. T.

3. *Electro-magnetic Rotation of the Plane of Polarization of Light in Gases.*—H. A. KUNDT and H. W. C. RÖNTGEN continue their research upon this subject, using a Gramme machine instead

of a battery. They remark that the current from the latter was sufficiently constant. In order to show, however, the fluctuations in current caused by irregularity in the contact of the brushes of the machine, and want of constancy in the speed of rotation, they enclosed a tube filled with bisulphide of carbon in a coil; polarized light was sent through this tube and the light was afterward viewed by means of a spectroscope. The apparatus was so arranged that a dark band appeared in the midst of the spectrum. This band or stripe moved to and fro with the fluctuations of the current. The authors state that this method is very suitable to convince one of the fact of variations in the current which the inertia of the galvanometer needle masks. The telephone has been used in America for the same purpose. It appears from their results that hydrogen, nitrogen and atmospheric air show nearly the same change of polarization. They do not find a simple relation between the rotation of the plane of polarization and the indices of refraction of the gases which were submitted to examination. The expression $\frac{d}{n^2(n^2-1)} = \text{a constant}$, in which d is the rotation, and n the index of refraction, an expression given by Becquerel (Ann. de Chim. et de Phys., (5) 12, 1877), they do not find to be correct, although they believe that the index of refraction has great influence upon the result; of no less importance, however, is the diamagnetic induction constant. The authors find that their later results with hydrogen and carbonic oxide agree with their earlier ones, but they do not find this agreement in the case of oxygen and atmospheric air.— *Wied. Annalen der Physik und Chemie*, No. 6, p. 257, 1880. J. T.

II. Geology and Natural History.

1. *Geological relations and fossil remains of the Silurian Iron ores of Pictou, Nova Scotia.*—Dr. J. W. Dawson, in the Canadian Naturalist (vol. ix, No. 6), gives an account of these ore beds with lists of fossils, and reaches the conclusion that the beds probably do not reach as far up as the Oriskany group (his former published view), but correspond in age with the upper beds at Avisaig, which are Upper Silurian. "These deposits of iron ore apparently began locally in an early part of the Upper Silurian period, and were continued into the Lower Helderberg period, while in the western part of Nova Scotia, in the Nictaux district, we have evidence of their continuation into the Oriskany age." There is no representative of the Niagara limestone in the region, a general fact in Nova Scotia, and along the Atlantic margin of North America, though present farther west in Northern New Brunswick, and in Gaspe.

2. *De Candolle's Phytography.* (Notice continued from the preceding (August) number.)—We proceed with our remarks upon this interesting volume, taking up certain points as they strike attention in turning the pages, but passing over many others of equal or superior importance.

There is a short chapter upon enigmatical descriptions or botanical riddles, and how they come about. The author has taken the pains to collect and tabulate the "species dubiæ" of the last four volumes of the Prodromus, to see who is accountable for them, taking into account only botanical authors no longer living, and excluding those who have contributed no more than three. So good a botanist as Blume heads the list, one so indifferent as Siebold is accountable for the fewest; so not much comes from such a tabulation. The practical point is that Blume, as well as Miquel and Kunth, who stand high on the list, have fallen much into the habit of founding species on fragmentary or quite insufficient herbarium specimens; instead of passing them over without mention, or at least without naming them. One is apt to suppose that a description of an incomplete specimen, say without flowers, may be readily eked out later by another hand supplied with the missing parts. This, as DeCandolle says, is a mistake. The succeeding botanist is hindered more than he is helped by such work. And it is the same with species founded on figures, such, for instance, as those of Moçino and Sesse, upon which the elder DeCandolle established species and some genera in the earlier volumes of the Prodromus. As to "*genera dubia vel non satis nota*," very few can be laid at the door of first class botanists. In the list of names of deceased botanists which are notable for their absence, the name of Torrey is inserted between that of the elder DeCandolle and that of the elder Hooker.

In the chapter on the description of groups superior to species, the author enumerates and sketches the character of the six *Genera Plantarum* which have appeared within the 180 years of modern botany; the immortal works of Tournefort (*Institutiones*, 1700), Linnæus (the first edition of whose *Genera* was published in 1737), A. L. Jussieu (1789), Endlicher (1836–1840), Meisner (1836–43, which is much less known), and finally of Bentham and Hooker, which began in 1862, and is now three-quarters finished. Tournefort fixed the rank and character of genera, Linnæus tersely and clearly defined them, Jussieu arranged them under natural orders, defining these; Endlicher and Lindley developed the hierachy of groups superior to orders, also the tribes inferior to them, and the latter is deservedly praised for his sagacity in discerning affinities, the former for the perfection of his style; and to Bentham and Hooker is justly awarded the crowning merit of having, far beyond their predecessors in this century, verified or developed the characters of the genera by a wide and direct study of the herbarium materials.

Floras, or descriptions of natural groups, not in their entirety but so far only as represented in a particular country or region, are discussed in Chapter X. In floras, as in more general works, abridged descriptions or diagnoses suffice, indeed are preferable in all cases where the region has been pretty well explored, and where the materials can be thoroughly elaborated. Formerly all considerable floras were written in Latin, at least the characters.

So they would continue to be if the convenience of botanists and the advancement of science only were to be considered. But floras are used by many to whom even Linnæan Latin would be a stumbling block. Fortunately the difference between good botanical English and botanical Latin is not wide, and will not seriously trouble a French or German botanist. The converse hardly holds. The greatest flora written in English, we might say the best great flora in any language which has ever been produced and completed, is Bentham's *Flora Australiensis*, in seven octavo volumes. Touching upon works of special illustration, the Botanical Magazine is justly singled out for praise, for sustained botanical correctness under difficulties, and for its great influence upon the science.

DeCandolle insists much on the importance of describing and well classifying the varieties of a species, and of distinguishing them as much as possible into grades, such as subspecies or races, varieties, subvarieties, &c. We suggest that this can be done with great advantage only when the forms are comparatively definite, or have been described as species. We think that only the more salient and definite varieties should be distinguished by names; otherwise the names and the groups will be limitless.

In the eleventh chapter, on partial descriptions of groups from the point of view of organography (a term which our author prefers to morphology), of physiology, botanical geography, &c., our author has some pertinent remarks upon the helps which all such studies are offering to phytography, which will gradually extend its domain over them; and upon the obvious advantage and great need of having results of histological researches expressed descriptively, under something like a common terminology, and with due regard to rules which have governed the more matured branches of botany,—rules and practices which eliminate a deal of verbiage, facilitate comparison of views, and ensure mutual intelligibility. Of botanical descriptions for the purposes of systematic botany, it could be said that whatever is not clear is not botany. May such clearness be hoped for in the future of histological botany?

Chapter XII treats of the unavoidable mixture of artificial with natural grouping. Truly natural groups are often artificially defined, that is, are indicated by single characters; or truly artificial characters are used for the sake of convenience in the division of natural groups. Of the latter sort are the divisions *Polypetalæ*, *Gamopetalæ*, and *Apetalæ* in Dicotyledons; also those founded on the mode of curvature of the embryo in *Cruciferæ*, introduced by Brown, who cautiously used them for genera, but raised to the rank of primary or subordinal characters by DeCandolle. Hypogyny, perigyny and epigyny are in the same category, and probably no one was more sensible of it than Jussieu himself, whose point and forte was the constitution of orders, not their collocation under these artificial heads. DeCandolle suggests that, while to the more natural divisions are appropriated the terms of

Class, Cohort, Orders, Tribes, Genera, and Sections; such names as Division, Subdivision, Series, &c., might be restricted to artificial divisions, and that these should take adjective names not of generic origin, such as *Liguliflorœ*, *Polypetalæ*, and the like.

Chapter XIII relates to difficulties in phytography which have grown out of various methods or absence of method in the nomenclature of organs, and from the want of consideration of the law of priority in such matters. The result of which in some departments, such as histological morphology, is a state of anarchy not unlike that which prevailed in the names of groups before the days of Tournefort and Linnæus. We may hope that order and lucidity will some day dawn upon this chaos and a common language replace this confusion of tongues. Meanwhile DeCandolle offers certain counsels, the utility of which, he says, is not doubtful nor the application very difficult.

(1) Hold fast to common and universally known names, whether in Latin or in modern languages. *Radix*, *caulis*, *folium*, *flos*, &c., with their vernacular equivalents, are not to give place to new-fangled substitutes. This, he thinks, will rid us of "such useless terms as *caulome*, *phyllome*, &c." Now these terms, along with *trichome*, seem to us legitimate and useful, as succinct expressions of a morphological idea; they are annoying only when pedantically ridden as hobbies over ground on which they are not wanted.

(2) Do not entertain the idea that a change in the mode of considering or defining an organ requires a change of name. Although Linnæus did take the leaf-blade for the leaf, and define it accordingly, that did not much hinder the coming in of a truer view, involving merely a change of the definition. But one may intimate that DeCandolle here comes into conflict with another rule he insists on, namely, that terms should have unmistakably one meaning. When we say—as we ever shall—that leaves are ovate, we speak according to the Linnæan definition; when we say that their insertion is alternate, we use the word in a more comprehensive sense; when we have occasion to declare that cotyledons, bracts, petals, &c., are leaves, we use the word in the most comprehensive sense. All this involves considerable ambiguity; and the endeavor to keep the new wine in the old bottles causes no little strain. It is borne because it has been applied gradually. If Linnæus had started with, or even reached our ideas, we should happily have had a nomenclature to match. Now we must be content, for descriptive purposes, to employ some words both in a restricted and in a comprehensive sense, and let the context fix the sense, just as it must in ordinary language. Technical precision is only a matter of degree. But it is clear that the excellent rule here laid down need not forbid the introduction of terms to express our conceptions, such as *rhizome*, *caulome*, *trichome*, and the like. Yet these are ill-chosen terms, except the last. In particular, *rhizoma* has long ago been appropriated for something which is not of root nature, but the contrary.

(3) The third counsel is to change the name of an organ, as we do that of a genus or species, only when it is positively contrary to the truth, or when it has been pre-occupied.

(4) Avoid giving special names for rare or ill-definable cases of structure. An epithet or short periphasis is vastly preferable to a new and strange term, which will be seldom used and may be hardly understood. DeCandolle truly remarks that after a a great multiplication of terms and distinctions generally comes some good generalization, which does away with a crowd of particular names; that what has happened in carpology is likely to occur for microscopic organs; and he adds: "Nous assistons au 'feu d'artifice' d'une trentaine de noms de ces états des cellules" [in our vernacular, we have seen them "go up"]; "il en restera seulement quelques-uns généraux ou fréquents, qui seront toujours nécessaires."

(5) Between two or more names choose, not the most agreeable, or even the most significant, but the one best known and most widely recognized.

(6) Between names equally known and used, adopt the oldest. Which are the older names is not difficult to know in the case of common organs; but is very much so in modern histology.

(7) In this matter of priority or of usage, consider only names taken from [or in conformity with] Latin or Greek. As in systematic botany, scientific and not vulgar names are to be accounted in this regard. Those who like *spaltöffnung* for *stoma* or *stomate*, and *scheitelzelle*, must needs follow their own fashion; but the genius of our own and the French language resists their importation, while it adopts or adapts with ease technical terms from classical sources.

(8) Not to admit names contrary to these rules.

Chapter XIV surveys some difficulties in phytography which arise from the variant, changed, or contradictory use of certain botanical terms, and from the employment of vernacular terms which cannot be latinized. The latter has just been referred to incidentally. Even the French describe the dehiscence of a certain kind of capsule as "*en boîte à savonette.*" In English we do not attempt to say "in soap-box fashion," and should not be understood if we did, but we adopt the Linnæan Latin "circumcissile." In general, DeCandolle concludes that a vernacular term, whether the name of an organ or of a botanical group, which will not enter into a Latin text by a modification of its termination, is not scientific, and may give place to one which is.

A few terms are mentioned which have been more or less changed in meaning since the time of Linnæus; such as *lanceolate*, which has gradually varied more or less, and for a part of the change the present writer is held to account; also *glaucus*, which classically means sea-green in hue, but which has been generally used in botany to designate sometimes a certain whitishness, and sometimes a whitishness caused by a minute waxy exudation in the form of a powder: the latter is the same as *pruinosus*. Others

may be as surprised as we were to learn that neither *glaucus* nor *pruinosus* are Linnæan terms.

Among the terms used ambiguously, it is surprising that DeCandolle does not refer to *pistillum*, first introduced into botany by Tournefort, and used in the sense of the modern term *gynœcium*, therefore only one to a flower; modified by Ludwig to denote a female member of the flower (having ovary, stigma, and commonly a style), of which there may be several or many in a flower; and adopted in the latter sense by Linnæus, yet generally with a use that avoids contradicting the sense of Tournefort. Mirbel, Moquin-Tandon, and St. Hilaire among the French, have openly departed from Tournefort's use, and speak freely of pistils in the plural. Brown and DeCandolle have used the word in the manner of Ludwig and Linnæus when they have used it at all, but have generally evaded its use; other botanists, especially British, have gone back to the Tournefortian sense of gynœcium. The present writer has a note on the subject in the new edition of his Structural Botany (1879 and 1880), p. 166.

Sinistrorse and *dextrorse* in the direction of ascent of climbing stems or the overlapping of parts in a bud, &c. DeCandolle had formerly insisted upon the desirability of following what he takes to be the authority and practice of Linnæus in the use of these terms; and he here returns to the subject, reinforcing his former arguments. It is most desirable that these terms should not continue to be employed in contradictory senses, one party calling that sinistrorse which the other calls dextrorse; it is also fitting that the principle of priority should prevail, and that the authority of Linnæus should be respected. Let us, therefore, in the first place give an abstract of the points which DeCandolle here makes.

But first, we take it for granted that a stem or such organ, having no front or back, can have no right or left of its own: so when we say that it twines to the left or right, we can mean nothing else than the right or left of the observer. The contradiction comes from the different position which the observer is conceived to occupy. DeCandolle supposes the observer to be placed within the coil or ascending helix, and that this is the more natural position. The other party supposes the observer to face the object from without; and from this position the Hop twines to the left, i. e., turns in ascending from the observer's right to his left, while the Convolvulus turns from his left to his right; the first is sinistrorse, the second dextrorse; while to DeCandolle, standing within the coil, the first is dextrorse, the second sinistrorse. Now, says DeCandolle, Linnæus in the first edition (1751) of the *Philosophia Botanica*, § 163, page 103, says: "Sinistrorsum hoc est quod respicit sinistrum, si ponas te ipsum in centro constitutum, meridiem adspicere; dextrum itaque contrarium."

DeCandolle remarks that the phrase "*meridiem adspicere*" is of no account [but it indicates a certain confusion in Linnæus's mind], for it matters not in what direction you look. He adds

—what we had all overlooked—that in the errata, on p. 360, Linnæus corrected the word *sinistrum* into *dextram*. But, inasmuch as two editions of the *Phil. Bot.* were printed at Vienna in Linnæus's life-time, and this correction was not introduced into them, he concludes that the correction was canceled by the author of it. And he notes that the expression "sinistrorsum hoc est quod respicit dextram" is a most awkward one for denoting the right-about change which the erratum had in view. Nevertheless the correction was so made in the edition of the *Phil. Bot.* by Gleditsch in 1780, two years after the death of Linnæus, also in that of Willdenow, published ten years later. But DeCandolle the elder, in the *Flore Française* and in all his writings, followed the original text, as also has the present DeCandolle, who cites as maintaining the same view, Braun (who for a time gave way to the opposite), Bischoff, Mohl, Palm, Dutrochet, Nægeli, and even Darwin. But we should say that Darwin, noting the conflict of views, had carefully evaded both, using instead the expression "with the sun, and against the sun." Yet sometimes saying "from left to right," as equivalent to "against the sun" (as on p. 34), showing that he took the external position to be the natural one.

Among those who have used the terms sinistrorse and dextrorse and defined them in the way which supposes the observer to stand outside of the helix, are Aug. St. Hilaire, Duchartre, Bentham and Hooker, Eichler; and the present writer may be added, although our author appears not to be aware of it. While trusting that the younger botanists will follow the example of Linnæus and the majority of authors, DeCandolle recommends that those who depart from it, and even those who adopt it, shall state their point of view by some convenient abbreviation, such as *extus vis.* or *intus vis.;* and thus lessen the danger of a misunderstanding. This is indeed essential.

DeCandolle remarks that he can discover no reason for the *ab extra* point of view except a tacit but perhaps nowhere expressed assumption that it requires some effort to suppose one's self in the center of a helix or spire. He thinks a moderate effort will accomplish this. The reply may be that, in the case of a stem climbing a hop-pole, or of the scales imbricated on the axis of a pine-cone, or of a flower-bud on the stage of a dissecting microscope, the contemplation of the object from without calls for no effort at all! So natural does this extraneous position appear to be that we found ourselves describing these objects from that point of view without thinking of any other,—so natural, as we shall see, that Linnæus fell into it himself, and there remained. Yet, that the opposing view has also its fitness is obvious from the fact that the physicists and mathematicians are divided in usage, no less than the naturalists.

In the actual state of the case, the question which view ought to prevail in botany must be determined on a balance there of considerations: 1. priority and authority, such as that of Linnæus;

2. naturalness; 3. preponderant actual usage. We had maintained in this Journal (for March and for May, 1877) and in Structural Botany (6th ed., note on pp. 51, 52) that the *externe visum* view has decidedly the best case on the second ground, and except in botany on the third also. And now that DeCandolle has drawn our attention to the matter, we are going to claim the remaining ground likewise, and to contend that the contrary usage in botany came in from non-attention to the teaching and practice of Linnæus himself!

On p. 39 of Linnæus's only own edition of the *Philosophia Botanica* he defines and illustrates the directions of twining thus: "*Sinistrorsum*, secundum solem vulgo: *Humulus, Helxine, Lonicera, Tamus. Dextrorsum*, contra motum solis vulgi; *Convolvulus, Basella, Phaseolus, Cynanche, Euphorbia, Eupatorium.*"

Nothing is said about the position of the observer. But in every one of the examples of sinistrorse (*Helxine* being *Polygonum convolvulus*), the stem winds around the support passing from right to left of the observer confronting the coil; and in every one of the dextrorse examples (*Eupatorium* being *Mikania*) it winds in the opposite direction. That is, dextrorse and sinistrorse are used in the *externe visum* sense. On p. 103 the same is repeated, except that reference to the sun's apparent course is omitted and additional examples are added, most (but not all) of them accordant with the preceding. So far, it would seem that Wichura was not mistaken in his statement that DeCandolle had followed a different method from that of Linnæus. And this appears to be the whole case as respects direction of twining.

But on the same page, to "*Corolla sinistrorsum*," is appended the foot-note which has made so much trouble, viz: "Sinistrorsum hoc est, quod respicit sinistrum, si ponas Te ipsum in centro constitutum, meridiem adspicere; *Dextrorsum* itaque contrarium." That is to say, in defining the direction of overlapping of the parts of a perianth, Linnæus took the open flower instead of the bud, and proposed to look down upon it from above or within. Now it may well be that Linnæus subsequently perceived the contradiction between his terminology for overlapping and that for twining; and that his brief erratum, on p. 310, "pro *sinistrum* lege *dextram*," was intended to bring the former into congruity with the latter, which it does, but in an awkward way. Perhaps he saw the incompatibilty of the cited examples; in fact about as many of them accord with the outside as with the inside point of view. Anyway, the erratum is his own; it seems unlikely that he authorized its omission from the Vienna editions; and Gledistch and Willdenow should not be blamed for heeding his behest in their editions. For, so far as it goes, it tends to render their author consistent with himself. If Linnæus had revised the page himself, he would have left out the "*meridiem adspicere*," which has nothing to do with the matter, and doubtless he would have completed his assimilation of the direction of petal-obliquity or overlapping with that of stem-winding; and so the whole confu-

sion from which we are endeavoring to escape would have been avoided.

In adopting the external point of view—now fortified by original authority—it is well to note that we shall be in accord with the modern physicists and mathematicians, and also with common people. The ordinary screw, on which the thread winds from left to right of the confronting observer, and which is driven home by the semi-rotation of the hand and fore-arm from left to right, is everywhere known as the right-handed screw; and this, with the corkscrew, is taken as the norm and exponent of right-handed rotation by Clerk-Maxwell (Treatise on Electricity and Magnetism, i, 23), and by Sir Wm. Thomson.

The analogies which have been adduced in favor of the inside position are mostly drawn from objects which have a right and left of their own; a building, for instance, has a right and left side or wing because it has a front and a rear. The right side of an assembly presided over by an officer who faces the members is quite arbitrarily, but naturally, taken to be that on the right of the chairman. But the right hand figures on a drawing or engraved plate are taken to be those on the right hand of the observer, notwithstanding that the plate, having face and back, has a right and left of its own.

Chapter XV refers to certain difficulties which grow out of ambiguous terms of ordinary language; for example, the various meanings of the word (*fin*) *end* or *purpose*, and the ambiguities in the use of the terms *Nature*, *natural*, *supernatural* (which lead off into philosophy, but are here treated rather in reference to style of exposition); also the change which has occurred in the scope of the word *history* in natural science.

Chapter XVI is an interesting and pertinent one, upon the manner in which facts observed under the microscope are described, and on the great saving of space and advantage in clearness which would be gained by the adoption, for all matters perfectly capable of it, of the Linnæan descriptive style, and of Linnæan Latin. Extracts from the German of Schacht, the French of Payer, and the Italian of Gasparrini are given, and by their side a rendering in descriptive Latin; and the words and letters are counted. The German specimen so treated is diminished to considerably less than half the number of words and a little less than half the number of letters. The French simmers down to one-third the number of Latin words and less than half the number of letters; and in the French of descriptive botany to less than one-half. The Italian extract of 51 words and 256 letters is expressed in Latin of Linnæan form by 21 words and 127 letters.

Style in botanical works is discussed in Chapter XVIII, which all young botanists should study, especially the portion which treats of the admirable style of Linnæus. In speaking of botanical style in the modern languages, the author notices the great advantage which the languages of Latin stock have inherited, and which the English-writing botanists have acquired, of ready and

free use of Latin and latinized technical words by direct transference. Botanical French, English, and Italian, are contrasted with the German in this respect. Noting that the German of conversation inclines to be clear and sententious, while in botanical writings the words lengthen more and more and the sentences become badly involved, our author remarks that recently having read a couple of pages of Vegetable Anatomy, and feeling his brain somewhat fatigued with the frequency of such words as *Sclerenchymfäsergruppen*, *Gefässbundentwickelung* and *Entwickelungseigenthümlichkeit*, he asked himself if that was good German style. He then recollected that Goethe, one of the very greatest of German literary writers, was also a profound naturalist. He opened his *Metamorphose der Pflanzen*, read a page or so, and experienced a relief which he likens to that felt by a sea-tossed ocean voyager when the vessel suddenly glides into a quiet harbor.

Chapter XIX discusses the propositions to employ letters and figures, chosen arbitrarily or otherwise, to represent specific and generic characters,—repulsive contrivances, to which our author lends no countenance.

Chapter XX treats questions of orthography, abbreviations and signs, pagination, typography; the twenty-first chapter, of titles and indexes; both full of interesting details upon which we cannot touch, although we are longing to put in our oar.

Chapter XXII animadverts upon the tendency of certain modern cryptogamists to set all botanical rules at naught. The next gives advice about articles in journals, dissertations, and the like; the next treats of translations; another, of figures, and has many noteworthy remarks; Chapter XXVI, of auxiliary and bibliographical works; and Chapter XXVII, is a chronological table of the progress of phytography, beginning with a Chinese encyclopedia 1000 years before Christ, and ending with Sachs' Lehrbuch, 1868–1877. Botanical students will find it very interesting and instructive.

The remaining Chapter begins the second part of the volume, *Preuves des Descriptions;* which is principally devoted to herbaria, their history, formation, and management;—a most important chapter, the analysis of which would form an article by itself. Last in order and not least in importance, a full enumeration is given of botanical collectors and authors who have formed herbaria, with an indication of the place where their herbaria or collections are preserved. A. G.

3. *Occurrence at Newport, R. I., of two littoral species of European Shells not before recorded as American;* by A. E. Verrill.—In the latter part of July and in August, of this year, I found living among the decaying sea-weed, at high-water mark in the docks at Newport, R. I., numerous specimens, both full grown and young, of *Truncatella truncatula* and *Assiminea Grayana.* They were associated with *Alexia myosotis*, *Anurida maritima*, *Chernes oblongus*, a large species of *Ligia*, *Orchestia agilis*, and other littoral species. Whether these shells have been

accidentally introduced, at that point, by shipping, or are really indigenous, cannot at present be determined. They are now certainly well established inhabitants of our shores. They may have been overlooked hitherto.

4. *Rapid diffusion of Littorina littorea on the New England Coast;* by A. E. VERRILL.—It is well-known to American conchologists that this common European species has become well-established on the New England coast within ten or twelve years, appearing first on the coast of Maine about 1868; Dr. Dawson, however, states that he collected it on the shores of Nova Scotia at a much earlier date. I wish, at present, merely to put on record some additional data, as to its recent progress along the coast. In 1873, it was collected, in abundance, at Saco, Maine, by the U. S. Fish Commission, and was found sparingly at Peake's I., Casco Bay. In 1872 it was very rare at Provincetown, Mass., but in 1875, it was common there. In 1875, it was collected by the writer at Barnstable, Mass., on the shores of Cape Cod Bay, in large quantities. In 1879, it had become exceedingly abundant at Provincetown. In 1875, our parties found two specimens only, on the southern shores of Cape Cod, at Wood's Holl, but in 1876 it was found to be common there, and is now very abundant. The first specimen found so far westward as New Haven was obtained by Professor S. I. Smith, during the past winter. Other solitary specimens have since been obtained here by Mr. E. A. Andrews, and by Mr. J. H. Emerton. It is, at present, exceedingly abundant at Newport, R. I.

5. *Artificial propagation of the Spanish Mackerel* (*Cybium maculatum*); by A. E. VERRILL.—That this highly valued fish habitually breeds at certain localities in Chesapeake Bay was recently ascertained by Mr. R. E. Earll, of the U. S. Fish Commission. In July, he visited the locality and made experiments upon its artificial propagation. He was very successful and easily hatched many thousands of the young fish. These, though among the most minute of larval fishes, proved to be hardy and easy to transport. The eggs hatched in less than 24 hours after fecundation. The U. S. Fish Commission will undoubtedly be able to utilize this discovery next year on a large scale, and there is every reason to believe that this excellent fish may be thus introduced into all the waters south of Cape Cod, in great abundance.

6. *Occurrence of Ciona ocellata* (*Ascidia ocellata* Agassiz) *at Newport, R. I.;* by A. E. VERRILL.—This ascidian, which is one of the largest and most elegant found on our coast, occurs in abundance at Newport, both on the rocks and on the piles of wharves, at low-water, and on dead shells, to the depth of 20 fathoms. It seems to be very local in its distribution, for I have never seen it at any other locality on our coast. It was originally obtained by Agassiz, at New Bedford, Mass., according to Binney, in Gould's Invert. of Mass., where it is figured, but not described. It grows to the length of four or five inches, and about an inch in diameter. It is very translucent, allowing the internal organs to be well

seen through the pale greenish or yellowish-white test. It is usually attached by the base and lower part of one side. The apertures are surrounded by a circle of bright lemon-yellow, and the ocelli are bright red. There are also two bright red spots connected with the nervous ganglia. The *Ciona tenella* (Stimp.), which is common in the Bay of Fundy, has the circles around the apertures bright red.

7. *Note by* C. A. WHITE.—In the August number of this Journal, p. 158, Mr. R. E. Call calls attention to two slight errors in my article "On the Antiquity of certain Subordinate Types of Fresh-water and Land Mollusca;" referring to the geographical distribution there attributed to *Unio complanatus* Solander, and to my use of the name "*Unio gibbus* Barnes." The latter is so plainly a typographical error that, if inexcusable, it is in no danger of misleading any one. My statement in relation to the former question was based upon the identification as *Unio complanatus* Solander of certain shells (collected by myself in Iowa) by the late J. G. Anthony. I accepted that decision without serious question because other species of branchiferous mollusks are well known to inhabit both Atlantic and Gulf drainage waters. Perhaps Mr. Call is correct in his opinion, but he gives it with an emphatic assurance which experienced naturalists seldom express upon such subjects, and in view of such facts. No fact in natural history is really unimportant, but as my only object in that paper was to show the antiquity of the types referred to, the question of the actual geographical distribution of a particular species of one of those types is not essential in that connection, however important it may be in connection with the subject of the origination of those species.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The American and British Associations* meet this year on the same day of August—the 25th, the former at Boston, the latter at Swansea. The President of the American Association is LEWIS H. MORGAN; the Vice-Presidents, ASAPH HALL, of Section A, and ALEXANDER AGASSIZ, of Section B. The President of the British Association is Dr. A. C. RAMSAY, Director-General of the Geological Survey, and the General Secretaries are Captain DOUGLAS GALTON and Dr. PHILIP L. SCLATER.

2. *Deep-Sea Sounding and Dredging: A description and discussion of the methods and appliances used on board the Coast and Geodetic Survey Steamer "Blake,"* by CHARLES D. SIGSBEE, Lieut. Commander U. S. Navy. 208 pp. 4to, with 41 plates. Washington, 1880. U. S. Coast and Geodetic Survey, Carlile P. Patterson, Superintendent.—This valuable volume describes in full detail, and with a profusion of excellent illustrations, the methods and appliances employed for deep-sea sounding and dredging on board the steamer "Blake."

The work of Lieut. Commander Sigsbee, on the "Blake" was carried on for four years beginning in the autumn of 1874. It

has resulted not only in the accumulation of much important data from soundings—12,766 nautical miles of sounding-lines, with serial temperatures, were run by him in the Gulf—but also in the invention by him of many new forms of apparatus and the improvement of previous ones, calculated to facilitate the operations of sounding and dredging. Prof. Alexander Agassiz was connected with the expedition of the Blake in the winter of 1877–1878, from December to March, and also during its later cruising, having had special charge of the collections from the dredgings, and has already published part of his results. The Superintendent of the Coast Survey, Captain Carlile P. Patterson, says with reason, in the preface:

"Without specifying the great results obtained from this continuous research, I may be pardoned in referring with some gratification to the fact that in the small steamer 'Blake,' of only three hundred and fifty tons burthen, N. M., under the energetic and skillful commands of Lieutenant-Commander Sigsbee and Commander Bartlett, with a complement of forty-five including officers and crew, more rapid work was done than had been accomplished with the old methods and appliances by the 'Challenger,' a vessel of over 2,000 tons burthen, with a complement of twenty-nine naval and civil officers and a correspondingly large crew." He adds with reference to the present volume, "There being no special publications with detailed instructions on the systems and methods adopted for deep-sea sounding and dredging, although much attention is now paid by all maritime nations to the subject, it has been thought advisable to publish the methods used on board the 'Blake.' These methods, in even so small a vessel as the 'Blake,' have been prosecuted with celerity, ease, and precision, showing that deep-sea work has become nearly as ready of accomplishment as ordinary littoral soundings."

The plates are partly heliotypes; and all details are given so as to make the work a full exposition of the best methods of deep-sea dredging, and of keeping records of the observations and working up the results.

OBITUARY.

Louis François de Pourtalès, died at Beverly Farms, Mass., in the 57th year of his age, on the 17th of July, 1880. Spite of a magnificent constitution and a manly vigor of body and mind, which seemed to defy disease and to promise years of activity, he sank after a severe illness under an internal malady.

Educated as an engineer, he showed from boyhood a predilection for natural history. He was a favorite student of Professor Agassiz and when his friend and teacher came to America in 1847 he accompanied him and remained for some time with the little band of naturalists who, first at East Boston and subsequently at Cambridge, shared his labors. In 1848 Pourtalès entered the U. S. Coast Survey, where his ability and indefatigable industry were at once recognized, and he remained attached to that branch

of our public service for many years. He there became deeply interested in everything relating to the study of the bed of the ocean. Thanks to the enlightened support of the then Superintendent of the Coast Survey, Professor Bache, and of his successors, Professor Pierce and Captain Patterson, he was enabled to devote his talents and industry to the comparatively new field of "Thalassography" and the biological investigations related to it. The large collections of specimens from the sea bottom accumulated by the different hydrographic expeditions of the U. S. Coast Survey were carefully examined by him and the results were published in advance of their appearance in the Coast Survey Reports in Peterman's Mittheilungen, accompanied by a chart of the sea bottom on the east coast of the United States. So interesting and valuable were the results obtained, not only as an aid to navigation, but in their wider bearing on the history of the Gulf Stream and on the distribution of animal life at great depths, that in 1866 he was sent out by Professor Pierce, then Superintendent of the Coast Survey, to continue these investigations on a larger scale. During 1866, 1867 and 1868 he was in charge of the extensive dredging explorations carried on by the U. S. Coast Survey steamer "Bibb," acting Master Platt, along the whole line of the Florida Reefs and across the Straits of Florida to Cuba, Salt Key and the Bahama Banks. The results of these expeditions, published in the bulletin of the Museum of Comparative Zoölogy, excited great interest among zoölogists and geologists. Mr. Pourtalès was indeed the pioneer of deep-sea dredging in America, and he lived long enough to see that these expeditions had paved the way not only for similar English, French and Scandinavian researches but had led in this country to the "Hassler" and finally to the "Blake" expeditions under the auspices of the Hon. Carlile Patterson, the present Superintendent of our Coast Survey. On the Hassler Expedition from Massachusetts Bay through the Straits of Magellan to California he had entire charge of the dredging operations. Owing to circumstances beyond his control the deep sea explorations of that expedition were not as successful as he anticipated.

At the death of his father Mr. Pourtalès was left in an independent position, which allowed him to devote himself more completely than ever to his zöological studies. He resigned his official connection with the Coast Survey and returned to Cambridge, where he became thenceforth identified with the progress of the Museum of Comparative Zoölogy. To Professor Agassiz his presence there was invaluable. In youth one of his favorite pupils, throughout life his friend and colleague, he now became the support of his failing strength. The materials of the different deep-sea dredging expeditions, above mentioned, had been chiefly deposited at the museum in Cambridge, and were thence distributed to specialists in this country and in Europe. A large part of the special reports upon them have already appeared. Mr. Pourtalès reserved to himself the corals, halcyonarians, holothurians

and crinoids. A number of his papers on the deep-sea corals of Florida, of the Caribbean Sea, and of the Gulf of Mexico have appeared in the museum publications. He had begun to work at the magnificent collection of halcyonarians made by the "Blake" in the Caribbean Sea, and had already made good progress with his final report on the holothurians. The crinoid memoirs published by him relate to a few new species of Comatulæ, and to the interesting genera Rhizocrinus and Holopus.

The titles of his memoirs indicate the range of his learning and his untiring industry. His devotion to science was boundless. A model worker, so quiet that his enthusiasm was known only to those who watched his steadfast labor, he toiled on year after year without a thought of self, wholly engrossed in his search after truth. He never entered into a single scientific controversy nor even asserted or defended his claims to discoveries of his own which had escaped attention. But while modest to a fault and absolutely careless of his own position, he could rebuke in a peculiarly effective though always courteous manner, ignorant pretentions or an assumption of infallibility.

Appointed keeper of the Museum of Comparative Zoölogy after the death of Professor Agassiz, he devoted a large part of his time to the administration of the museum affairs. Always at his post, he passed from his original investigations to practical details, carrying out plans which he had himself helped to initiate for the growth of the institution. As he had been the devoted friend of Professor Agassiz he became to his son a wise and affectionate counselor without whose help in the last ten years the museum could not have taken the place it now occupies.

If he did not live to see the realization of his scientific hopes he lived at least long enough to feel that their fulfillment is only a matter of time. He has followed Wyman and Agassiz, and like them has left his fairest monument in the work he has accomplished, and the example he leaves to his successors. A. AG.

Cambridge, Mass., July 31, 1880.

Professor E. B. ANDREWS, of Lancaster, Ohio, one of the corps of geologists engaged since 1869 on the Geological Survey of Ohio, and the author of a very valuable volume among its final reports, as well as various geological memoirs, died on the 21st of August, in his sixtieth year. He was a graduate of Marietta College, Ohio, and its Professor of Geology from 1851 until he entered on his work in connection with the State Geological Survey. During the five years previous to 1851 he was pastor of churches in Housatonic, Mass., and New Britain, Conn. The State of Ohio owes much to him for his careful study of the coal deposits of the southeastern part of the State—the part under his special charge,—and for other labors connected with the development of its mineral and geological resources. Mr. Andrews was highly esteemed for his many excellencies, and will have his place in scientific history for his part in the progress of American geology.

General ALBERT J. MYER, the efficient head of the United States Signal Service, died in Buffalo, on the 24th of August, in his fifty-second year.

This number of the American Journal of Science not only is much enlarged in consequence of the demand of its long articles, but fails of several book notices which should have appeared in it. The following are among the volumes received.—

Report of the Superintendent of the U. S. Coast Survey, showing the progress of the work for the fiscal year ending with June, 1876. 416 pp. 4to, with 24 maps. Washington, 1879.

The Natural History of the Agricultural Ant of Texas: a monograph of the habits, architecture and structure of Pogonomyrmex barbatus, by Henry Christopher McCook. 312 pp. 8vo. Philadelphia, 1880. (J. B. Lippincott & Co.)

The Geological Record for 1877: An account of works on Geology, Mineralogy, and Paleontology published during the year, with supplements for 1874–76. Edited by William Whitaker, B.A., F.R.S., of the Geological Survey of England. 432 pp. 8vo. London, 1880.

Contributions to Paleontology, Nos. 2–8, by C. A. White, M.D. [Extracted from the 12th annual Report of the U. S. Geological Survey, 1878, F. V. Hayden, U. S. Geologist in charge.] Author's edition. 171 pp. 8vo, plates 11 to 42. Washington, 1880.

Annals of the Astronomical Observatory of Harvard College, vol. xii: Observations made with the Meridian Circle during the years 1874 and 1875, and prepared for publication under the direction of Joseph Winlock and Edward C. Pickering, successive Directors of the Observatory, by William A. Rogers, Assistant Professor of Astronomy in the Observatory. Printed from the Sturgis fund. 271 pp. 4to (with introduction i–xcii). Cambridge, 1880.

Catalogue of 618 Stars observed at the Astronomical Observatory of Harvard College, with the Meridian Circle during the years 1871–2, 1874, 1875. [Extracted from vol. xii of the Annals, see above.]

First Annual Report of the Department of Statistics and Geology of the State of Indiana, 1879. 514 pp. 4to. Indianapolis, 1880.

Elementary Treatise on Electric Batteries. From the French of Alfred Niaudet, translated by L. M. Fishback. 266 pp. 8vo. New York, 1880. (John Wiley & Sons.)

Science Primers: Introductory by Professor Huxley, F.R.S. 94 pp. 12mo. New York, 1880. (D. Appleton & Co.)

Water Analysis for Sanitary Purposes; with hints for the interpretation of results, by E. Frankland, Ph.D., etc. 144 pp. 8vo. Philadelphia, 1880. (Presley Blakiston.)

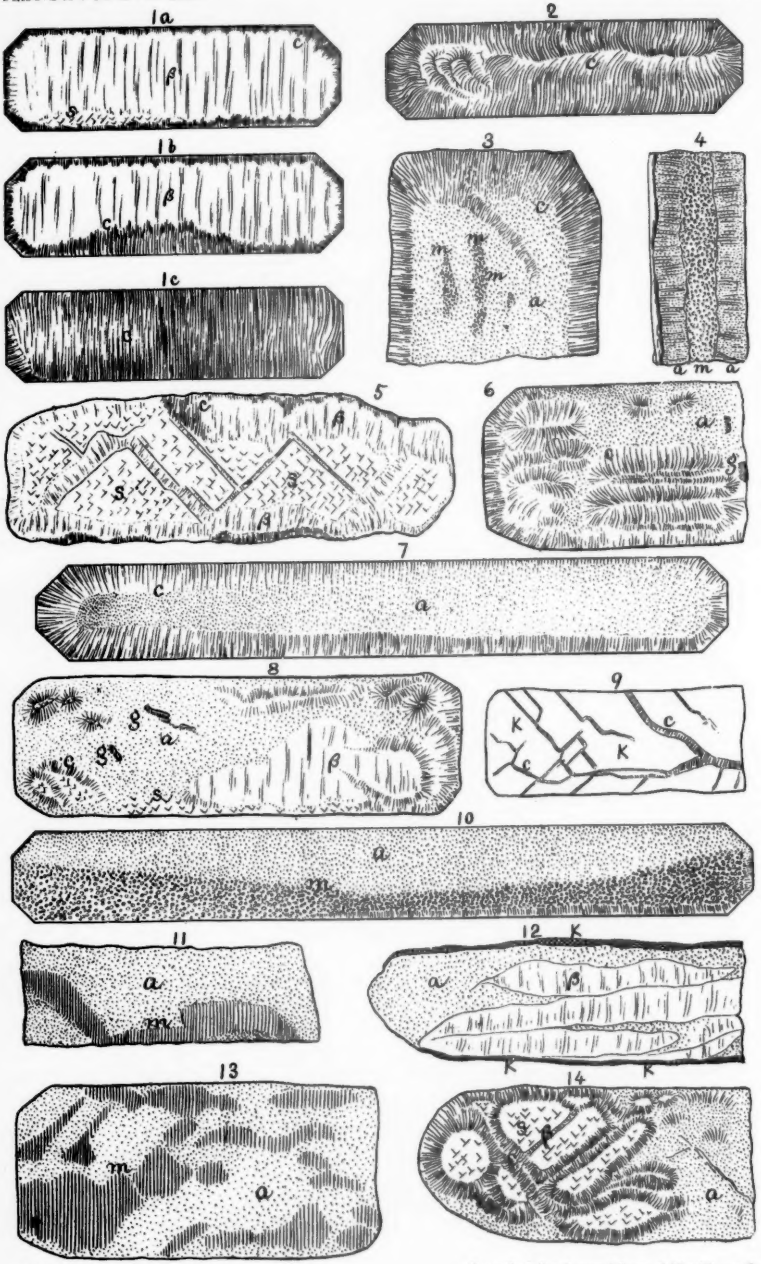
Interesting Chemical Exercises in Qualitative Analysis for ordinary schools, by Geo. W. Rains, M.D. 59 pp. 8vo. New York, 1880. (D. Appleton & Co.)

Proceedings of the National Microscopical Congress, held at Indianapolis, August 14–19, 1878; and of the American Society of Microscopists, held at Buffalo, N. Y., August 19–24, 1879. 77 pp. 8vo. Indianapolis, 1880.

The Microscopists Annual for 1878. Number 1.—containing useful tables, rules, formulæ, memoranda, list of Microscopical Societies, directory of prominent makers, etc. 48 pp. 12mo. New York, 1880. (Industrial Publication Company.)

Regenwaarnemingen in Nederlandsch-Indië. Eerste Jaargang 1879 door Dr. P. A. Bergsma, Directeur van het Observatorium te Batavia. 257 pp. 8vo. Batavia, 1880.

Mémoires sur les Terrains Crétacé et Tertiares préparés par feu André Dumont. Pour servir a la description de la Carte Géologique de la Belgique. Édités par Michel Mourlon. Tome iii, Terrains Tertiaires, seconde partie. 459 pp. 8vo. Brussells, 1878. Musée Royal d'Histoire Naturelle de Belgique.



PSEUDOMORPHS AFTER SPODUMENE.